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## FIFTY YEARS OF CHANGE IN AMERICAN PSYCHOLOGY

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From time to time attempts have been made to define the scope ✓ of psychological science or to trace shifts of professional interest by classifying the contents of psychological periodicals. The principal American studies of this type are those of Fernberger (13, 16) Ruckmick (28), and Buchner (6). For the most part these analyses have dealt merely with the *titles* of articles or counted the entries under the rough rubrics employed in the *Index* or *Abstracts*. By contrast, the present study disregards titles and delves into the body of each article to determine in a detailed way the author's problem, his presuppositions, procedure, explanatory concepts, and his outlook on psychological science.<sup>1</sup>

To obtain a liberal perspective in time and an adequate sample, the entire periodical output of the "leading" psychological journals for every tenth year beginning in 1888 and ending in 1938 was

<sup>1</sup> Besides classifying the contents of periodicals, writers have employed a variety of other devices for securing historical perspective. Some have counted the number of laboratories operating in the United States at different times (9, 20, 22, 27); some have enumerated the research interests reported by American psychologists (3, 12, 16) or computed the volume of their published studies (7, 8, 10, 11, 14, 15, 17, 18); others have traced the geographical distribution of psychologists in America (3, 4, 12, 13, 24) or listed the subjects taught by them (3, 12, 20, 22, 27). Still other sources have been explored more or less systematically (5, 23, 24, 32). Many reminiscent accounts of past achievements and changes in psychology are on record (7, 19, 25, 29, 30, 31). The number and variety of these historical reviews and professional self-assessments demonstrate that psychology is indeed a self-conscious discipline.

included. To determine the "leading" journals, a list of 50 American psychological and quasi-psychological periodicals was submitted to 30 members of the American Psychological Association, with a request that ratings be given on a scale from 0 to 3 to indicate each journal's "significance for, and devotion to, the advancement of psychology as a science." Ratings of 3 indicated journals of "central significance, containing many important contributions"; 2, "fair significance, containing some fairly important contributions"; 1, "minor significance, containing some articles of minor value"; 0, "of no significance to the advancement of psychology as science." The 14 journals heading the list in respect to mean ratings were chosen for analysis; all received mean ratings of 1.5 or higher. There follows a list of these journals with the dates of their founding:

- American Journal of Psychology* (1887)
- Psychological Review* (1888)
- Pedagogical Seminary* (1891)
- Psychological Bulletin* (1904)
- Journal of Abnormal and Social Psychology* (1906)
- Journal of Educational Psychology* (1910)
- Journal of Comparative Psychology* (formerly *Journal of Animal Behavior*) (1911)
- Journal of Experimental Psychology* (1916)
- Journal of Applied Psychology* (1917)
- Journal of General Psychology* (1928)
- Journal of Social Psychology* (1930)
- Character and Personality* (1932)
- Journal of Psychology* (1935)
- Psychometrika* (1936)

Excluding from the survey all book reviews, personal notes, letters to the editor, and other such incidental material, there remained a total of 1627 articles in these 14 journals distributed in time as follows:

1888-1898.....	86 <sup>2</sup>
1908.....	145 <sup>3</sup>
1918.....	314
1928.....	346
1938.....	736
Total .....	1627

<sup>2</sup> Because of the small number of articles in each year, the results for these two decades have been combined throughout this study. In order to augment the sample for the first decade, Volume I of the *Pedagogical Seminary* (1891) was included.

<sup>3</sup> To secure a larger sample in this decade, Volume I of the *Journal of Educational Psychology* (1910) was included.

## RUBRICS FOR CLASSIFICATION

A preliminary survey of this body of literature suggested a large number of pertinent categories by which the analysis might be guided. Some of these groupings, however, proved to be nondifferentiating or else unreliable in application. There survived the following 32 categories, to which all articles were ordered according to the criteria described below. Theoretically an article could be entered under all 32 categories or under none at all. In practice it was rare for an article to be listed under more than 15 or less than 5.<sup>4</sup>

- (1) *The use of normal human adult subjects.* (92.5)
- (2) *The use of child and adolescent subjects.* (99.1)
- (3) *The use of abnormal human subjects.* Studies on psychotics, neurotics, toxemic and glandular cases, etc. are included here. (92.5)
- (4) *The use of subhuman animal and vegetable subjects.* (98.1)
- (5) *Empirical studies in physiological psychology.* Included are studies of the physiology of emotion, motivation, sensation, learning, etc., regardless of the technique used—electroencephalographic, surgical, chemical, or other. (97.2)

<sup>4</sup> Note on reliability. All of the categorizing was carried out by one judge, J. S. B. In order to determine "observer-reliability," G. W. A., employing the same rubrics and the same criteria, categorized a sample of 107 articles independently. The work of the two classifiers was then compared article for article with respect to (a) common inclusions, (b) common exclusions, and (c) disagreements. By this method a measure of agreement was obtained for each category and for all categories taken together. Agreement is defined as the percentage of common inclusions and exclusions. The following table gives the percentage agreement for all the categories taken together by years:

Year	Number of Articles	Percentage Agreement
1908.....	17	92.6
1918.....	28	92.4
1928.....	26	91.0
1938.....	36	93.3

Comparisons were made article for article in the following way: Each category was considered 107 times by each observer; i.e. an article was marked for either inclusion in or exclusion from each category, and there were 107 articles. The percentage agreement is the number of times the articles were marked for inclusion by both classifiers plus the number of times they were marked for exclusion by both, divided by 107, the total number of articles. Agreement for all the categories taken together is obtained by determining the mean of the agreements for the individual categories; this figure, representing our final reliability measure, is 92.2%. In parentheses following the description of each of the rubrics is given the percentage agreement of the two classifiers for the rubric in question.

(6) *Physiological references.* Any mention of sensory physiology, neural mechanisms, physiological foundations of mental processes, etc., provided it in some way sustains the argument of the paper, qualifies an article for inclusion here. Out-of-context references do not, e.g. a life-history article which mentions the fact that the subject had a glandular imbalance as a child and does not use that fact in any other way than as part of the physical history of the case. (86.0)

(7) *Higher mental processes studied through nonlinguistic responses.* Any investigation of higher mental processes (excluding perception but including learning) is listed here provided it does not call for verbal report from the subject. Animal experiments make up a large proportion of these entries. Excluded are studies of human subjects employing standardized performance tests. (100.0)

(8) *Higher mental processes studied through linguistic responses.* This division includes every investigation of verbal learning, reverie, creative thinking, concept formation, and any other H. M. P. that calls for the use of language by the subject. Excluded are studies based on mental tests. (88.8)

(9) *Higher mental processes studied through standardized mental tests.* All investigations of the H. M. P.'s employing either verbal or performance tests are included here; the majority of these have to do with the measurement of some kind of mental capacity. (96.3)

(10) *Introspective studies.* This group comprises experimental investigations that depend wholly or in part upon introspective methods. Introspection is here defined as contemplation or reflection upon an experience, either while in progress or in memory, with a careful and full description of that experience (adapted from Warren's *Dictionary of psychology*). (92.5)

(11) *The use of statistics.* This category includes all articles that employ statistical aid, ranging from simple measures of variability and correlation to complex factorial procedures. But simple statements of central tendencies are not included under this heading. (95.3)

(12) *Empirical studies in social psychology.* This is a loosely defined field, including such studies as attitude testing, group influence on the behavior of individuals, effects of culture on behavior, communication, public opinion, etc. (92.5)

(13) *Social problems.* To be classified here an article must mention some social problem explicitly. (94.4)

(14) *Applied psychology.* Theoretical or experimental contribu-

tions aimed primarily at the direct application of psychological principles to life outside the laboratory qualify for inclusion here. (88.8)

(15) *Cultural reference.* Articles utilizing the concepts of cultural relativity, acculturation, cultural determination, cultural frames of reference, etc. are classed here; likewise papers discussing social change. (78.5)

(16) *Empirical studies of motivation.* This category includes the psychological study of human and animal motivation (subjective as well as behavioral); likewise physiological studies of the basis of motivation. (89.7)

(17) *Empirical studies of personality.* Listed under this category are paper-and-pencil personality tests, clinical studies, and all other investigations ordinarily classified under "the psychology of personality." (87.9)

(18) *Articles dealing with the single case.* Included in this category are: (a) case histories, biographies, autobiographies, diaries, etc. that attempt to obtain an *understanding* of the total personality in its *milieu*; (b) analyses of artistic and literary creations oriented toward an understanding of the personality of the creator or some mental characteristic of the creator; (c) intensive studies of one culture or social institution. Most of the psychological writings of anthropologists are included in this group. (87.9)

(19) *General reviews of the literature.* These may take the form of a long survey article of the type featured in the *Psychological Bulletin*, or may be comprehensive historical introductions to empirical or speculative articles. (93.5)

(20) *Facultative treatment of mental and neural functions.* This category includes only those studies offering explanations in terms of facultative entities. Among them are found papers that endeavor to "explain" phenomena by invoking (a) instincts; (b) brain centers; (c) symbolic mechanisms to which are ascribed the power of a *deus ex machina*; (d) libido when invested with explanatory force; (e) modern factorial concepts if construed as "mental powers," e.g. the Spearmanian use of *g*; (f) the faculty of "attention"; and (g) other miscellaneous facultative concepts. (91.6)

(21) *Explicit allegiance to some school of psychology.* In order to qualify under this category, an article must defend a school of psychology in such a context that it is implicitly or explicitly placed in contradistinction to an alternative school or system. Articles simply referring to the concepts of some school as a basis for setting

up experiments, but overlooking the broader systematic implications of this procedure, are not included. (95.3)

(22) *Explicit attacks upon schools of psychology.* These attacks are usually on the grounds of "mysticism," "metaphysical contamination," "verbiage," etc. Examples of this type of criticism are found in the writings of psychologists who have been influenced by the modern positivistic movement. (96.3)

(23) *Concepts of dynamic segregation.* This category includes all discussions dealing with the differentiation of phenomena or events within a totality. Included are such conceptions as individuation (both psychological and physiological), figure-ground, field theory, activity within the context of the total personality, situational determination of social events, and various principles of Gestalt dynamics. (86.0)

(24) *Explicit manifestations of the unconscious.* There are two criteria for classifying an article here: (a) the use of the concept of the unconscious in an *explanatory* manner; (b) the treatment of some phenomenon as a manifestation or reflection of the unconscious or of an unconscious mechanism. (85.0)

(25) *Treatments of the body-mind problem.* Under this heading are subsumed (a) papers attempting the solution of the problem either in monistic or dualistic terms and (b) papers attempting to solve the problem by asserting that it does not exist. (97.2)

(26) *Specific mental process treated as entity.* An article which contains speculations stemming from such a question as "What is the nature of intelligence?" is classed here. The presupposition of such speculation must be (explicitly or implicitly) that the process dealt with is a "real" one and not merely fictional or operationally defined. (67.3)

(27) *Specific mental process treated as construct.* This category differs from the preceding in that it includes speculative articles that view some specific psychological process as a formal construct rather than as an entity. These papers attempt to answer such questions as: "What is the status of the construct 'intelligence' in such and such a system of psychology?" or "What properties must the construct 'intelligence' have in order to be consistent with other constructs in such and such a system of psychology?" (98.1)

(28) *Postulational and geometric methods.* To be included here an article must make use of an explicitly stated set of postulates or axioms and attempt to deduce consequences through manipulation

of logical or geometrical symbols according to the rules of the postulate system. (100.0)

(29) *Statistical methodology.* Included here are *discussions of* (not merely the application of) statistical methodology, derivation of new statistical formulae, etc. (99.1)

(30) *Mentions of operationism.* Articles which explicitly mention operationism as a criterion for scientific definition and procedure constitute this group. (100.0)

(31) *Treatments of the problems of the formal methodology of psychology.* Included in this category are the following: (a) discussions dealing with the nature of psychological data and the methods of approaching these data, whether experimentally or conceptually, e.g. "The data of psychology are the higher mental processes and the method of approach to these data is strictly phenomenological"; (b) examination of psychological definitions and constructs *on a formal level*, e.g. attempts to examine a definition in respect to its consistency, linguistic structure (logical syntax), extensionality, operational denotation of "intervening variables"; (c) discussions of the general problems of scientific method; and (d) analyses of the formal problems of specific fields of psychology, e.g. discussions of the meaning of Vincent curves in learning experiments or the use of the phi-gamma hypothesis in psychophysics. (91.6)

(32) *Formal conceptual analysis (a composite).* Any article which is entered in *one or more* of the following categories is listed here: mentions of operationism (30); problems of formal methodology of psychology (31); speculations on the formal, conceptual nature of psychological constructs (27). This composite category was devised to get an index of the growth of what may be called "methodological positivism." (93.5)

How much does an article have to say about a topic covered by a category in order to be included under it? No quantitative answer can be given to this question. One of the aims of the investigators was to guard against the dangers of simple "nose-counting." The inclusion of an article under a category depends upon the judge's interpretation of the *intention of the author* rather than upon the presence or absence of certain words or phrases. In some instances, an article was placed in a certain category if it contained as few as two succinct sentences on the categorical topic. In other cases classification would be based on 10 paragraphs or more of discussion.

Classifying an article in one category sometimes means that it must also be classed under another. For example, all articles under (5) must also be classified under (6), and all articles under (25) must also be classified under (26) or (27). Category 32, of course, is based entirely on cross-classifying.

### RESULTS<sup>5</sup>

The findings reported in Table I pertain to the use of various kinds of subjects in psychological investigations. Note that in all decades the normal adult human is the preferred subject. His falling off in popularity in 1908 seems to reflect a temporary falling off in empirical research of all kinds, compensated for by an attendant rise in speculative studies. This peculiarity of 1908 is further demonstrated by the entries under Categories 21, 24, 25, and 26 (Table III).

TABLE I  
PERCENTAGES OF INVESTIGATIONS UTILIZING VARIOUS KINDS OF SUBJECTS

Category	1888-1898	1908	1918	1928	1938
(1) Normal adult human...	48.8	24.9	30.6	50.8	42.9
(2) Child and adolescent...	24.5	13.8	16.9	23.8	15.4
(3) Abnormal human .....	2.3	8.3	4.1	2.9	3.3
(4) Animal.....	3.5	4.1	8.9	9.0	15.2

The proportion of studies in the earliest decade based on work with child or adolescent subjects is greater than for any subsequent period, a fact that is due, as the articles themselves show, to the influence of G. Stanley Hall. As in the case of adult human subjects, 1908 reflects a general decline in the proportion of articles using children or adolescents as the subjects of research. After reaching a second peak in 1928, the figures show a decline in the proportion of child and adolescent studies. Category 3 calls attention to the relatively small percentage of psychological investigations utilizing abnormal human subjects. The largest figure, in 1908, is due, no doubt, to the fact that the *Journal of Abnormal Psychology*, first included in this decade, plays a relatively larger part in the sample than in later years.

Perhaps the most significant feature of Table I is the uninterrupted rise in the use of animal subjects. Their popularity is demonstrated likewise by Fernberger (16), who reports that in the five years from 1932 to 1937 the number of articles dealing with animal psychol-

<sup>5</sup>A portion of these results formed the basis of the Presidential Address to the Forty-seventh Annual Meeting of the American Psychological Association (1). In this address some of the implications of the changes here reported were dealt with at greater length than in the present article.

ogy written by Members and Associates of the American Psychological Association was surpassed only by the number of articles written in the field of "general experimental psychology." In Fernberger's data it is noteworthy, however, that although the animal articles rank second in number, a relatively small number of psychologists do research in this field—a tribute to the productiveness of the few. G. W. Allport (1) has pointed out that 25% of the papers delivered at the annual meeting of the American Psychological Association in 1939 were based upon animal research; whereas in 1914, 25 years earlier, the corresponding percentage was 11.

TABLE II  
PERCENTAGES OF ARTICLES CONCERNED WITH VARIOUS FIELDS AND  
TECHNIQUES OF INVESTIGATION

Category	1888-1898	1908	1918	1928	1938
(5) Physiological experiments . . . . .	7.0	4.1	1.3	7.8	11.1
(6) Physiological references . . . . .	26.7	32.4	17.5	31.3	29.2
(7) H. M. P. nonverbal methods . . . . .	2.3	4.8	8.0	11.3	13.6
(8) H. M. P. verbal methods . . . . .	22.1	13.8	11.8	13.3	6.1
(9) H. M. P. testing methods . . . . .	0.0	6.9	17.8	16.2	9.5
(10) Introspection . . . . .	2.3	4.1	3.5	5.5	1.6
(11) Use of statistics . . . . .	2.3	5.5	27.4	45.5	43.6
(12) Studies in social psychology . . . . .	7.0	4.1	2.9	6.4	6.5
(13) Social problems . . . . .	12.8	21.4	6.7	7.5	3.0
(14) Applied psychology . . . . .	24.4	42.7*	33.4	25.2	18.6
(15) Cultural references . . . . .	20.9	14.5	9.2	16.2	9.5
(16) Motivation studies . . . . .	5.8	1.4	2.6	8.7	7.9
(17) Personality studies . . . . .	16.3	4.8	5.4	9.3	15.5
(18) Single case studies . . . . .	16.3	7.6	4.5	6.7	4.9
(19) Reviews of literature . . . . .	5.8	11.0	14.3	17.9	7.3

\* The large percentage of articles on applied psychology in this year is due, in large part, to the inclusion of the vigorously practical first volume of the *Journal of Educational Psychology* (1910), which played a relatively larger rôle in the output of this period than in later years.

Table II represents the changing interests of psychologists with respect to subject matter and with respect to certain techniques of investigation. Category 5, dealing with investigations in physiological psychology, mirrors the tendency of Category 4: as the use of animal subjects increases, so too does interest in physiological investigation. Physiological investigations and references (Categories 5 and 6) both show a trough in 1918. In that year writers utilized fewer physiological concepts than in any other year in our survey. One explanation lies in the practical demands of the war, which drew psychologists' attention to problems of psychotechnics and personnel—a fact that is demonstrated by the prominence of testing and applied psychology in that year (Categories 9 and 14). Another explanation may lie in the shift that was taking place in 1918 away from a naively exuberant faith in the possibility of finding easy physiological

solutions to all psychological riddles, and towards a critical attitude regarding standards required for work in this field. Within the past 20 years there has been a marked improvement of proficiency in physiological experimentation and a gradual recovery of interest and assurance. Fernberger (16) found that physiological psychology has steadily risen in rank, among the research interests of members of the A. P. A., from twenty-seventh place in 1918, to thirteenth in 1927, and to seventh in 1937.

Category 7 shows that, with the rise in the use of animal subjects and in physiological interests, there is an attendant increase in the study of the higher mental processes by the use of *nonverbal* methods. Psychologists are showing a preference for referring problems of learning, reasoning, and concept formation to animal subjects or to human subjects who for the duration of the experiment are totally mute. Correspondingly, the use of verbal report or protocols based on introspection, as shown in Categories 8 and 10, has undergone a striking decline. In the light of the enormous importance of verbal behavior to all higher mental processes, the falling off of psychologists' interest—until in 1938 only 6.1% of the total research output bears on higher mental processes involving language—is worthy of special note.

Excluded from Category 8, however, are those investigations that attempt to study cognition by the use of standardized tests (Category 9). Obviously, the study of cognitive ability by this method limits the investigator to problems of mental capacity and individual differences, since it cannot be made to yield information concerning the *nature* of the active processes that underlie performance: insight, creative thinking, abstraction, etc. The impetus given by the war to applied psychology in general and mental testing in particular seems to have carried over to 1928, but thereafter to have declined.

The entries under Category 10 indicate that the pages and pages of critical discussion devoted to the problems of introspection were scarcely justified by the slim output of articles based on strictly introspective procedures. In all years the figures are low.<sup>6</sup>

Quantitatively, the most striking change in 50 years is the great increase in the use of statistical aids in psychological research, shown

<sup>6</sup> In Ruckmick's analysis of American psychological periodical literature from 1905 to 1915 (28), there was a greater proportion of so-called introspective articles recorded than in the present study. No comparison can be made, however, as Ruckmick included under the rubric "introspection" all perception studies, even those based on the merest discriminative response. We have used a narrower, more strictly defined criterion for classification.

in Category 11. In part this rise reflects the growing proficiency of psychologists in the use of techniques of critical investigation, and in part, no doubt, it reflects the preoccupation with quantitative standards of excellence characteristic of most American cultural activities—even science.

Categories 12-19 deal with various types of subject matter that occupy American investigators to a greater or lesser extent. Though Category 12 shows no significant quantitative change in interest in social psychology, close scrutiny of the articles themselves reveals an interesting qualitative shift. Early studies in this field were dominated by evolutionary theory as expressed in the influential works of Hall and Baldwin, whereas the past two decades have shown virtually none of this type of influence, but have reflected, rather, the influence of capacity and habit psychology, and, quite recently, the influence of various theories of cultural determinism. Fernberger (16) has found that ranking of research interest in social psychology among Members of the American Psychological Association has risen slowly from eighth place in 1918 to fifth in 1937.

Category 13 shows a marked falling off in the extent to which authors discuss social problems in psychological periodicals. Similarly, Category 14 discloses a decline in contributions applying psychological principles to life outside the laboratory. In part these declines seem to be due to the more exacting standards required for publication that have grown up in recent years. It is intrinsically more difficult to observe scientific requirements of adequate controls, quantitative precision, and appropriate conceptualization in the complex fields of social and applied psychology. The result is that many psychologists who would prefer to deal with problems that are near to life find it more convenient and more conducive to professional reputation to publish specialized experimental articles conforming to current standards rather than to their personal tastes and aspirations.<sup>7</sup> No doubt the decline in Categories 13 and 14 may be explained in part by the fact that the 14 journals in our survey, having come to prefer purely theoretical and experimental papers, are forcing authors of practical papers to publish them in a variety of occupational, political, and other semipsychological media.

These explanations may also account for the dropping off in the

<sup>7</sup> What, for example, are we to make of the fact, discovered by Fernberger (16), that among the 1551 Associates of the American Psychological Association in 1937 clinical psychology ranks first as a research interest, when a negligible number of these researches get published in the "leading psychological periodicals"?

number of references to cultural factors (Category 15). The temporary rise in 1928 seems to be due to the prominence in that year of interest in the nature-nurture controversy, which prompted several investigations of foster children, raciocultural differences, etc.

Category 16 reveals augmented concern during the past two decades with problems of motivation, a result apparently of three converging trends: interest in the physiology of emotion; in the rôle of motivation in learning; and in the dynamic theories of Freud, McDougall, Kempf, and others.

Category 17, dealing with personality, mirrors some of these same tendencies as well as the influence of mental test methods. The rise is steady from 1908 onward. The high figure for 1888-1898 shows, as do so many entries for this decade, the influence of G. Stanley Hall and his methods of investigating development.

Although there can be no doubt about the recent growth of interest in personality, the percentages under Category 18 show a general decline of concern for the single case. Personality is studied more frequently, but, paradoxically enough, the individual is studied less often. Most psychologists do not regard clinical cases, individual life histories, or single historical events as appropriate material for professional publication.

The recent decline of systematic historical reviews reflected in Category 19 is attributable to three possible factors: (a) lessened respect for past research; (b) editorial pressure to shorten articles to fit crowded journals; and (c) the ever-increasing difficulty of preparing historical reviews in the common fields of investigation where research accumulates at a rapid rate. Whatever the reason, there has been an abrupt swing away from historical-mindedness in recent periodical literature.

Table III deals with theoretical issues and with various modes of conceptualization. It is not surprising to see proof under Category 20 of a downward swing in facultative approaches. The entries reach a low point in 1928. Very few writers then were willing to invoke instincts, brain centers, attention, or other "mental powers" to explain their data or bolster their theories. The slight rise in 1938 is due to the inclusion of certain neofacultative representations of mental "factors." Not all the investigations employing factor analysis are classified here, but only those that clearly identify factors with mental powers, *e.g.* the Spearmanian use of *g*.

Categories 21 and 22 include articles representing explicit allegiance to, and explicit attack upon, schools of psychology. Consider-

ing the prominence that systematic issues hold in psychological discussion, the percentage of periodical literature devoted to them is notably small. Probably the case for and against schools is too long to be stated in the journals, and is generally reserved for treatises. The relatively high interest in schools in 1908 and 1928 will be interpreted later.

Category 23, though somewhat difficult to define, reflects what is undoubtedly a significant trend. Although the common opinion is that the last decades of the Nineteenth Century were marked by

TABLE III

PERCENTAGE OF ARTICLES REFLECTING VARIOUS MODES OF CONCEPTUALIZATION

Category	1888-1898	1908	1918	1928	1938
(20) Facultative treatments .....	19.2	12.4	5.4	3.8	4.2
(21) Allegiance to schools.....	2.3	8.3	2.6	7.0	2.9
(22) Attacks on schools.....	0.0	0.7	1.9	4.6	2.5
(23) Dynamic segregation .....	12.8	6.2	5.7	13.1	16.0
(24) Manifestations of the unconscious.....	10.5	15.2	7.3	7.5	5.8
(25) Body-mind problem .....	5.8	7.6	2.9	2.0	2.0
(26) Specific mental process treated as entity .....	22.1	34.5	10.8	13.9	7.2
(27) Specific mental process treated as construct .....	3.5	5.5	2.6	5.8	5.6

allegiance to *elementarism*, our analysis shows that American writers during these years paid at least a lip-service to the principles of totality. There were, for example, many references, though unsystematic in nature, to the "total self," the "cultural *milieu*," "levels of neural activity," etc. From a low point in 1918 there has been a steady rise in the systematic use of these principles due, unquestionably, to the combined influence of Gestalt psychology and modern neurology.

In view of the Freudian influence on psychology, it may seem surprising that the entries under Category 24 (manifestations of the unconscious) show a decrease. This fact is probably to be explained (a) by the disinclination of psychologists to use the somewhat facultative language of the unconscious; and (b) by the habituation of psychologists to this mode of explanation until at the present they often seem to accept the activity of the unconscious without stating their explicit dependence on it. (Explicit dependence would be required to qualify an article for inclusion in Category 24.)

Papers dealing with the body-mind problem, Category 25, have fallen off in number, but most of all they have changed in character.

Few writers now discuss the issue unless for the purpose of demonstrating its "illusory" nature. If an aside be permitted, we should like to record our impression that however negativistic most psychologists are to history and tradition in this matter, most of them adhere in effect to a psychophysical parallelism. (Cf. Kattsoff's discussion of Hull's conceptual system (21), for example.)

Various categories (viz., 7, 8, 20, 21, 22, 25) have indicated the trend away from "realism" and towards "nominalism" in psychology. More directly the point is substantiated by the entries under Category 26, within which are included those articles whose presupposition is that some certain psychological process is a real entity. Note that the percentage of such articles in 1938 is scarcely one-fifth the percentage in 1908. Category 27 does not in itself succeed in showing the rise of the nominalistic view of mental processes, since

TABLE IV  
PERCENTAGES OF ARTICLES DEALING WITH METHODOLOGICAL ISSUES

Category	1888-1898	1908	1918	1928	1938
(28) Postulational and geometric methods.....	1.2	0.0	0.3	1.5	3.3
(29) Statistical methodology.....	1.2	0.0	3.2	11.6	7.2
(30) Mentions of operationism....	0.0	0.0	0.0	0.3	2.0*
(31) Formal methodology .....	7.0	11.0	11.8	19.7	15.6
(32) "Methodological positivism"....	10.5	15.2	14.0	27.6	25.4

\* In the first half of 1939 this percentage had increased to 4.6.

the criteria for inclusion were narrow and limited. A more accurate representation of this trend is seen in Category 32, which combines several indices of the same order.

The evidence contained in Table IV clinches the argument that the concepts of modern psychology are nominalistic in character. Here we see clearly the unmistakable influence of logical positivism and its attendant movements. The use of postulational and geometric methods, Category 28, though still relatively rare, has increased; so, too, has the development of new and rationalized statistical methods (Category 29), though in this case there seems to be a diminution of interest between 1928 and 1938. Category 30 lists *mentions* of operationism (not necessarily systematic expositions of the operational point of view). Because of the recency and popularity of this concept in present-day psychology, the percentages for the first half of 1939 have been added. The absolute magnitude of these percentages clearly does not do justice to the power of this concept as a verbal catalyst in present-day psychology.

Category 31 is an embracing one, showing again the recent preoccupation of certain psychologists with various methodological problems. There are a large number of papers dealing with the nature of science, syntax, and other formal problems. To obtain an index of the positivistic trend under discussion, we have combined under Category 32 the percentages of Categories 27, 30, and 31. A compilation reveals that the percentage for 1928 and 1938 is nearly twice as great as the percentage for all the preceding years. Whether the falling off in 1938 as compared to 1928 is indicative of a turning of the tide, it is still too early to say, but it is well to keep this possibility in mind.

#### THE DESIGN IN EACH DECADE

Thus far we have dealt only with the upward and downward trends reflected in our 32 separate categories. The account has been one of change in as many distinct interests and practices. But besides reading our four tables *horizontally*, as we have done, it is equally possible to read them *vertically*, thereby obtaining an impression of the pattern characteristic of each of the five periods under discussion. [Unfortunately, no one has yet discovered a method for representing an historical epoch with strict accuracy. To state the relation of one event or trend to another seems inevitably to require a considerable measure of subjective interpretation. To be sure, our tables give some indication of the *weights* that should be assigned to one or another of the interests or activities present in each period. Yet the task of representing objectively and verifiably the subtleties of a *Zeitgeist* has thus far eluded workers in psychological and social science. The following interpretations are neither exhaustive nor completely verifiable, though they attempt to conform as closely as possible to the evidence contained within the four tables.]

1888-1898. The tone of this period was predominantly optimistic. Psychology was breaking away from philosophy—as the writers of the period delighted in assuring their readers—and was confidently building a house of its own. That a good deal of empirical research was being conducted is evident from a glance at Table II. Two trends are especially apparent: one, physiological and materialistic, deriving its background from Wundt and the German physiologists (as evidenced in Categories 5 and 6 and also in the large number of sensation studies not listed separately in this article); the other, humanistic (cf. Categories 8, 15, 17, 18), betraying considerable interest in clinical studies, single cases, culture, and personality.

The conceptual interests of the period were relatively naïve. Facultative instinct theories were rather generally accepted (Category 20); the treatment of mental process, consonant with the optimism of the period, was primarily "realistic" (Category 26). It was still possible for a psychologist to discuss the mind-body problem without being attacked from the point of view of some reductionist conceptual frame. The unconscious, even in those pre-Freudian days, was generally accepted.

There was a disregard for most of the problems of method, but some concern for the question, "What is the subject matter of psychology?", and for psychophysical techniques and for methods of measuring sensation.

Schools had not yet become particularly important. It was all one big happy family: disputes about functionalism, structuralism, introspectionism, objectivism had not yet become divisive.

The period gives the general impression of healthy growth with few signs of the uncertainties that plague us today; there was little doubt about the meaning of results, about the reality of each and every mental process that could be named, or about the possibility of making useful applications of psychological research. The period leaned heavily upon the Darwinian interpretation of social and individual development, with concomitant faith in the psychologist's and educator's ability to speed up and improve the evolutionary process.

1908. This was a year of ferment, marked by an upswing of theoretical disputes and an attendant decline in empirical studies. Functionalism was being opposed to structuralism, objectivism to subjectivism, imageless to palpable thought, and the unconscious to the conscious. Objectivistic psychology and the hope for monism were gaining ground, but there were many who fought against such betrayals of mentalism. The turmoil is reflected in various parts of our data: Table I shows 1908 to have the least empirical zeal of any of the years in our survey, while interest was at its peak in the body-mind problem (Category 25), in the unconscious (Category 24), in physiological references (Category 6), in process as entity (Category 26); and there were numerous protests of allegiance to schools (Category 21).

All in all, it was a theoretically-minded (but not a methodologically-minded) era. But though hot battles were waged, there was, on the whole, little skepticism regarding the possibility of ultimate realistic solutions. One felt that the objectives were practical. Social problems (Category 13) and applied psychology (Category 14) were

dealt with frequently, although the age of mental tests had not yet dawned, and the store of methods was meagre.

1918. Here we see psychology in wartime. Every one of the theoretical interests and disputes that characterized 1908 shows a marked decline; but the study of higher mental functions with testing methods (Category 9) and statistics (Category 11) leap upward, and applied psychology remains high. On all sides the practical demands of war service in the army, in industry, and in civilian life are evident. Psychologists say, in effect, why worry about the nature of intelligence when we must measure it *toute de suite* in a million cases? Why ponder the body-mind problem when our immediate task is to keep body and mind intact in the old-fashioned way?

The falling off in physiological studies in this period has already been explained in part as a consequence of practical war-demands, and in part as a reaction against the inexpertness of earlier dabbling in physiological research by untrained psychologists. It has also been remarked that 1918 was a period of elementarism. Interest in context and in dynamic segregation (Category 23) reaches its lowest point. Elementarism, too, may be a reflection of the hasty, segmental nature of the problems studied in wartime. Research was piecemeal and it was forced; the outpouring of tests is an example. Furthermore, molecular behaviorism was at this time near its zenith, and for most theoretical thinking of the period the reflex arc or the habit was the ultimate conceptual unit.

1928. Although marked by intense experimental activity, this year—like 1908—was one of vigorous controversy. Gestalt psychology was gaining ground, though not without stiff opposition from opposing schools (Category 21), from those who distrusted all schools (Category 22), and from those who preferred statistical counting to configuration (Categories 11 and 29). It was a period marked by nature-nurture controversies and by research with children and adolescents. Growing objectivism had shifted many theoretical discussions to methodology, and some psychologists were falling in line with neopositivism (Categories 31, 32). At the same time interest in the history of psychology ran high at this period (Category 19)—higher than in any other year of our survey. Motivation (Category 16) took a sudden upturn, and along with it physiological work (Categories 5 and 6) revived vigorously. The impression is one of diversity and vigor. Psychology had mounted the steed of prosperity and had dashed off in all directions at once. It was the predepression era, and the universe—so it seemed—was endlessly expanding.

1938. As yet there is not sufficient distance to enable us to tell whether this year marks a step in an ever-increasing positivism of American psychology or a period of transition away from positivism. Bills (2) has characterized the psychology of this period as *empirical*, *mechanistic*, *quantitative*, *nomothetic*, *analytic*, and *operational*. Although these epithets find ample justification in our tables, yet certain categories that might lend support actually show an appreciable decline as compared with 1928 (*e.g.* 11, 22, 29, 31, 32). Likewise it should be noted that studies in personality (Category 17) and in dynamic segregation (Category 23) have increased their proportions. The evidence, such as it is, seems to point to the existence of two opposed trends. Both of these trends represent reactions against historical tradition (*cf.* Category 19), and both seem related to the present critical state of world politics.

The first, shunning the floodlight of "social significance," seeks the salvation of psychology by adhering more and more tenaciously to rigorous methodologies derived from the natural sciences and from modern logic. Expressions of this trend are seen in the growth of operationism as well as in the upswing of physiological and animal investigations. The contrary trend is seen in the demand for a consideration of human values and for social research (Categories 12 and 17), and for fundamental concepts that have relevance to the problems of everyday life. (The S.P.S.S.I. and the A.A.A.P. were founded shortly before 1938.) What the outcome will be, no one can yet say. The official approval of the A.P.A., however, has recently been given to the cause of the "participationists" (26, p. 754):

"Be it resolved that the American Psychological Association, believing democratic institutions to be the indispensable foundation of free scientific inquiry into human behavior, record its conviction that earnest efforts should be made by American psychologists to study all those phases of American life and opinion which bear on the safeguarding of our fundamental liberties and peaceful way of life."

"Be it resolved, further, that this resolution be communicated to all Committees of the Association, to its Editorial Board, and to its representatives to Councils and to other organizations, with instruction that in so far as the functions of those bodies are in any way concerned with the issue herein stated, efforts be made to implement this resolution with appropriate action."

Yet not even the A.P.A. can legislate changes into the history of psychology. One may safely predict that the temperament of individual psychologists as well as the pressure of political events to

come will determine the ultimate effectiveness of the Association's resolution.

### SUMMARY

This historical investigation is offered as a contribution both to the study of social change and to the study of psychological epochs. By classifying all of the literature contained in 14 leading American professional journals at 10-year intervals, it was possible to trace the upward and downward trends in 32 types of psychological interest and activity. The longitudinal course of change as well as the cross-section patterns at 10-year intervals reveal a great complexity of factors. Though there is an impression of variegated unity at any one period of time, yet there is also distinctive change from one decade-interval to the next.

The year 1938 presents in fairly dramatic form two alternative courses for the future. Briefly, these courses might be designated as "psychology for science's sake" and "psychology for society's sake." These designations do not imply, however, simply a choice between pure and applied psychology, nor between isolationism and participationism. For the course selected will determine inevitably the design of mental science in the future—the character of its presuppositions, the systematic nature of its concepts, the structure of its theories, the fashioning of its techniques, as well as the content of its research.

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## EXPERIMENTS ON LEARNING IN SCHOOL SITUATIONS

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### INTRODUCTION

The educational psychologist faces in two directions, the immediately practical and the systematic. His research on the practical side may, and often does, take the form of a quest for some educational expedient in total disregard of the problems of learning and remembering as recognized in academic psychology; or he may concern himself with a repetition or simulation in a classroom of certain laboratory procedures in order to discover whether or not a particular finding may be made to apply in school. Over and above these practical considerations the educational psychologist has an obligation to general psychology.

When testing in school practice the discoveries of the laboratory, he is in a position to collect evidence toward the proof or disproof of the generality of the discoveries. Educational psychology could render a greater measure of service to education and psychology if more of the experiments conducted in its name were so designed with reference to laboratory investigations as to constitute tests of their generality. General knowledge is immensely practical.

On the whole, educators have entertained considerable skepticism regarding the pedagogical applicability of the experiments of the psychological laboratory. Tolerance might be increased by a realization of the fact that results, *qua* results, of the laboratory are not intended to be of value. The psychologist is interested in experimental results only in so far as they constitute clues to principles about which he may generalize. He is not interested in the number of repetitions required to learn a list of syllables to a criterion, aside from the variable conditions that influence the number.

This paper is not exhaustive. The writer trusts that the review is representative in the sense that the articles treated are typical. The literature is scattered over three or four decades and is to be found in more than a score of journals. The task of gathering and evaluating all of it would be tremendous; and the space for its publication would exceed by far that allotted to this paper. Moreover, the citing of evidence beyond a certain point may in some cases cease

to be a virtue. Use is made of no more than half of the material gathered. The educational psychologist who is dissatisfied with laboratory citations may relieve himself of the necessity of depending upon them by carefully searching the literature.<sup>1</sup>

#### INDIVIDUAL DIFFERENCES

A tremendous amount of incidental data is available on grade-overlapping in achievement.

A few sample investigations are those of Buckingham (28), Elliott (61), Gray (90), and Starch (209, 210). Ayres (9) appears to have been the first to employ grade-overlapping as a method of describing individual differences in achievement (1909) as well as among the first to study individual differences in academic achievement by any method. In 1918 Kruse (134) published the results of an investigation planned specifically to ascertain the amount of grade-overlapping in the elementary school. Among the few recent investigations is that of Cornell (43). Taking the various elementary-grade subjects singly, we find that approximately a third of the pupils in a given grade equal or exceed the median of the next grade above, and that only a slightly smaller percentage fall at or below the median of the next grade below. Conservatively, 10 to 12% deviate from the modal grade by two full grades above or below; and 2 or 3%, by three grades.

Cornell (43) found the grade range of 7-year-old pupils to be from 1 to 6, the middle 80% spreading over one and one-half grades. Her sampling of 10-year-old pupils ranged from standard second-grade to standard ninth-grade performance, the middle 80% spreading over three grades, 3A to 6A.

*Sex Differences.* Lincoln (140) has given a very competent summary of the literature on sex differences drawn largely from the cases used in standardization of achievement tests (1927). Of special interest in this work also is the extensive investigation of Brooks (21) in 1921. In arithmetic computation a consistent advantage is found in favor of girls; in arithmetic reasoning boys tend slightly to excel. Girls are somewhat superior in reading and spelling, and decidedly so in handwriting. In history and probably in all of the social sciences the advantage goes to the boys. In English composition the differences are small, but seem to favor the girls. In algebra boys appear to have a very slight superiority.

The wealth of literature that has appeared since the publication of Lincoln's book corroborates the foregoing summary, in the main.

<sup>1</sup> This paper is addressed to the educator as well as to the psychologist. Criticisms and questions of procedure that would be of interest primarily only to psychologists are omitted.

The results of the North Carolina High School Senior Examination, as reported by Jordan (121), give the boys an advantage in history and general science and a slight advantage in mathematics; advantage goes to the girls in English usage; while virtual equality obtains in reading and in literature.

With respect to reading, Jordan's results confirm those of Traxler (227) obtained at the high school level. In a comparison of several hundred boys and girls in high school physics, Hurd (109) found boys to be decidedly superior in knowledge of physics at the beginning of the course and somewhat superior at the end. Carroll (32) found high school and college girls to be decidedly superior to boys of the same classification in appreciation of literature. Foran and O'Hara (69) found boys to have a marked superiority over girls in geometry test scores.

Periodic analyses have been made of the scores achieved on various tests in the Iowa Every-Pupil testing program. These have appeared for the most part in masters' theses under the direction of E. F. Lindquist. A few are listed here. Kaskadden (124) found no differences as between boys and girls in ninth-grade algebra. DeKalb (47) obtained a marked difference in favor of boys in general science; Camp (29), a small difference in favor of boys in American government; Bane (10), a substantial difference in favor of boys in United States history; Tidman (224), a substantial difference in favor of boys in economics. Sperry (206) found girls to rank above boys in every section of an English correctness test, the advantages being rather marked. Of the foregoing, Kaskadden obtained no significant difference; Camp's critical ratio was only 2.6; the other differences are fully significant. (8, 58, 66, 143, 171)<sup>2</sup>

*Effect of Practice.* The effect of equal amounts of training upon individual differences was first investigated by Thorndike (218) in 1908. It has seemed important in educational psychology from various standpoints. For example, Hahn and Thorndike (95) stated that since "equalizing opportunity does not seem to equalize achievement, . . . nature, not nurture, seems to be the chief cause of the differences in ability" with respect to the performance in question. To Reed (180) the issue has certain important implications for education in a democratic state. It may be suggested also that a knowledge of the effects of equal amounts of practice upon the individual differences should prove to be of value in any consideration of selection in education. Recently Woodrow (250, 251) has made "improvement with practice" upon the part of subjects of varying ability levels the basis of a factorial study of abilities.

From the beginning, the question of method has been troublesome. Thorndike treated his results by two methods: (a) the ratios of the

<sup>2</sup> Numbers inserted thus refer to additional articles listed in the bibliography dealing with the same general topic.

slower to the quicker learners on the initial trials and on the final trials, (b) the correlations between initial scores and gross gain. In addition, Whitley (242) recommended (a) mean deviation, (b) mean deviation divided by the mean, and (c) the average deviation divided by the square root of the mean. A graphical method is sometimes used. This may be done by the amount-limit or time-limit method. In 1912 Wells (238) reported that diverging curves were obtained when plotted in terms of the amount of work done per constant unit of time. However, he pointed out that, when plotted in terms of the time required to do successively equal amounts of work, the curves converged. In two recent articles Skaggs (201, 202) has observed the same phenomenon. (179)

In general, when results are treated by the time-limit method individual differences are shown to increase with equal amounts of practice. When treated by the amount-limit method they "seem" to decrease. The latter appears to be a mathematical artifact.

Per cent of gain, coefficients of variability,  $r$  between initial scores and per cent of gain, or any other method using percentages seems inappropriate. We wish to know whether subjects are more alike or less alike after practice. The initially inferior usually gain by a larger per cent than the initially superior. This they do despite the fact that the latter usually make the larger gross gain—that practice increases the distance between the good and the poor.

The effect of practice on individual differences varies with the difficulty of the task, as Skaggs and others have shown. Chapman (34) obtained correlations of .20 between initial scores and gross gain in color naming and of about .40 in mental multiplication. In keeping with the same tendency Race (177) found that the initially superior (college students) made greater gross gain in addition and multiplication, while the initially inferior made greater gross gain in cancellation. There is also a suggestion in the work of Ruch (186) and Wilson (244) that the more highly performance on a task correlates with test intelligence the more are differences in performance increased by equal amounts of practice.

Henmon (102) administered a standardized arithmetic test to a group of pupils each month within the school year. He found a positive relationship between initial performance and gross gain. Reed (180) reports a well-executed experiment conducted by Edwards in which standardized achievement tests in various subjects were administered to Grades 2 to 8, inclusive, in October, February,

and May. Unfortunately, the results are stated in terms of correlation between initial scores and *per cent* of gain, coefficient of variability, and other methods involving ratios—all of which are almost certain to "show" a negative relationship between "improvement" and initial achievement.

#### SENSORY MODE OF PRESENTATION

The doctrine of image types began to make an impression upon educational theory around the beginning of the present century. It was assumed that the "visually-minded" pupil, for example, by virtue of his wealth of visual images, could make associative connections more readily when utilizing visual content than when utilizing some other sensory content; and that recall could be most readily effected by the modality in which he could command the most faithful images. Next it was assumed that material should be presented to the pupil through the sense that accorded with his dominant modality. The latter is probably questionable, even if the first assumptions and the doctrine of types itself be valid. Abbott (1) observed, as early as 1909, that subjects made easy transference from one modality to another and experienced no difficulty in recalling by images of one modality when the material was presented through a sense of different modality.

If it is found that pupils learn more readily when material is presented by one sense than by another, this fact may be utilized in teaching, quite aside from the question of image types. Incidentally, differences in sensory preferences would be much easier to determine than differences in image types.

In summarizing investigations in 1912, Henmon (101) wrote: "The results of the experiments are not in accord." Subsequent experimentation has yielded no results that warrant any modification of Henmon's pronouncement. However, the published investigations seem quite inadequate to the elimination of the possibility that systematic differences do exist among different individuals. Of course, we should anticipate that some subjects in a given instance will earn their best scores by one mode of presentation; others, by another. Errors of measurement would give this result. Whether or not consistent differences exist is unknown.

*Reading and Listening.* Young (255) compared listening with (a) reading at pupil's own rate and (b) reading for a period of time equal to that required by the teacher to present the material orally,

in the case of about 1200 pupils, Grades 4 to 6, and 15 different selections of text material. The median of the reliability coefficients of his tests is about .75, and the median of the correlations between reading and listening scores, about .65. Almost without exception the highest scores were obtained when the pupils listened to the reading of the selection; the lowest, when the pupils read at their own rate. The differences are not large and often are unreliable. Young's findings are in agreement with those previously obtained by Erickson and King (64). (49, 62)

*The Lecture Method.* The effectiveness of the lecture method has in general been investigated by two methods. One seeks to keep all conditions, such as content of the material, motivation, and size of class, constant, except the mode of presentation itself. In this form the problem is continuous with that discussed under the caption *Reading and Listening*. Although this method ostensibly yields to fairly exact experimental control, the controls tend to eliminate the problem.

A second procedure compares the lecture method with other methods such as informal class discussion, or the use of conferences, in which the content of the material and other conditions are admittedly different. It yields less readily to rigid experimentation, but it gets at a more significant aspect of the general problem.

Investigations by Greene (93) and Corey (41) utilize the former procedure; those of Spence (204) and of Remmers (182) the latter. Greene found that college students profited equally well from listening to the delivery of prepared lectures and from reading the same lectures during the class period, when tested immediately. On delayed testing the oral presentation proved to be somewhat superior. Corey, who allowed his subjects, college students, to read and study a selection of material for a period of time equal to that required by him to present it as a lecture, obtained an advantage in favor of reading.

Spence compared the achievement of two large college classes when taught by the lecture method and by the class-discussion method. A small advantage was obtained in favor of lecturing. Remmers, in an extensive investigation of college teaching, compared three methods of instruction: (a) the lecture method, with large classes, (b) the lecture-discussion method, in which large classes met twice a week and small discussion groups once a week, and (c) the discussion method with classes of 36 or less. By objective and essay tests, administered at the close of each semester, the discussion method proved to be slightly, but not significantly inferior to the lecture and lecture-discussion methods. The latter were equal to each other. The differences were somewhat greater on delayed tests. The students expressed a preference for the discussion method. (39, 99, 120)

### METHODS OF STUDYING

*The Effect of One and of Two Presentations.* If the question "How effective is a single reading?" is put in general form, it is, of course, unanswerable. The gain from a reading will vary especially with the nature of the material and the rigorousness of the examination. From a practical standpoint we may expect to find out approximately how much pupils learn of the assignments to which they are typically put, as measured by the prevailing methods; and whether or not, on the whole, the additional gain from a second reading is sufficiently large to justify its recommendation. Moreover, it would be helpful to know the level of mastery that should be insisted upon. Should pupils, in accordance with their several abilities, study only so much material as they can master, or should each read once as much material pertaining to the subject as he can in the time allotted? Every teacher must make a decision about this.

Almost all investigations of learning school subjects give some indication of how much is learned in terms of the measures used. Yoakam (254), among others, has made this a point of special investigation. He sought to ascertain the effects of a single silent reading of various types of material in common use in the elementary school. He concluded that elementary pupils learn, on an average, less than half of the total ideas in the articles read, and often only a third or less. Actually, he alleges, his tests covered the more important ideas. (88, 148, 173)

Yoakam (254) also compared the effectiveness of a single reading with the effectiveness of two consecutive readings. His subjects improved their scores comparatively little by the second reading.

*The Effect of Recall.* Recall as a method of studying has engaged the attention of psychologists for three decades. An investigation by Katzaroff (125) appeared in 1908, one by Abbott (1) in 1909, one by Thorndike (219) in 1914, and one by Gates (77) in 1917. Examples of later investigations are the articles by Jones (120), Moose (155), Trow (228), and Skaggs, *et al.* (203). The trend of the results of these investigations is well known to every careful student of psychology and education. Unfortunately, almost all of the investigations have been limited to memorization, and that of relatively short selections. Knowledge of the effect of recall in factual learning of text material and of the various conditions that influence it and feasible methods of implementing it in school are urgently needed. In school, pupils are required to do a good deal of memorization and learn a body of discrete material. Here both the facts and procedures of the laboratory may be expected to apply.

We do not know how reading and recall compare in factual learning, and we have no more than a few suggestions regarding the implementation of recall in school. To be of any substantial value, procedures for recall should be incorporated in the teaching program.

(1) *Implementing Recall.* Germane (83, 84, 85), in the case of grade school and college students, obtained a substantial advantage in favor of a "controlled mental summary" as opposed to a rereading of certain short articles. The pupils were instructed to answer mentally certain prepared questions. Bridge (20) compared four methods of "revision," all of which involved recall to some extent, namely: (a) filling in blanks, as in a completion test, (b) writing answers to questions and problems, (c) oral questioning and class discussion, and (d) oral summaries. Only the written response proved to be no better than no revision at all. Her general findings confirm an earlier investigation of Eaglesham (55).

The examination and class discussion afford two controllable means of implementing recall. Germane's "mental summary" is not easily controlled, though doubtless effective when coöperation is maintained.

*The Effect of Intensive and Extensive Study.* The writer has come across comparatively little experimentation on this topic. Good's (89) and Weaver's (237) results are indecisive, though tending to favor the extensive method. Rice's (184) recent article shows a substantial advantage in its favor. The latter also gives a good review of the existing literature.

*The Effect of Study Questions.* Washburne (234) investigated the effect of questions placed at the beginning and end of the story, and at the beginning and end of paragraphs, on the learning of facts and the making of generalizations in social science material. On the whole, the use of questions was fully justified. The optimal position was at the beginning of the story; the poorest, at the end. Placement at the beginning resulted in a gain in the facts and meanings covered by the questions with no loss in the learning of other parts of the story. The placing of questions at the beginning and end of paragraphs did result in some loss in learning facts not covered by the questions. The loss did not extend to meanings not so covered. The subjects were 1456 junior high school pupils. An investigation by Holmes (105) justifies the practice of having students (college) find the answer to questions as they read. (114, 126)

*The Effect of Distributed Study.* Perhaps no single recommendation is urged more often in educational psychology than dis-

tributed effort, and but few of the major topics have received less attention in school. Probably it is assumed that the daily character of the assignment and recitation is sufficient to insure the desired amount of distribution. If we may judge from other than school data, it would seem that the conventional lesson a day, or sometimes every other day, represents about the right interval between practices. To insure daily rather than fortnightly execution of the assignments, a short, daily quiz is recommended.

We do not know the optimal length of the recitation or study period. There is a good deal of administrative "waste"—10 or 15% of the school day at present—in changing from one class to another. Thus the optimal length of the work period as reckoned by yield per unit of time may not be the optimal from the standpoint of the total yield per day. (82, 214)

#### THE EFFECT OF EXAMINATIONS

The mere taking of certain kinds of examinations results in genuine learning—perhaps as much, per unit of time, as any other kind of activity. Incidentally, effective use of any teaching device or method of studying is difficult. There is no reason to suppose that pupils will automatically learn more when learning is facilitated; they may learn about the same and loaf more.

*The Examination Set.* Do students study any differently or learn any more under the impetus of one examination set than under that of another? Investigations by Meyer (151, 152, 153) suggest an affirmative answer. He obtained evidence that college students study differently for essay and completion (recall) tests than for true-false and multiple-choice ones, and that they learn more as evidenced by scores earned on all four kinds. (36, 45, 54, 139, 216, 217, 241)

*Learning Value in Taking Different Kinds of Examinations.* The data with respect to the essay and multiple-choice examinations indicate a preference for the latter. Bridge (20), Eaglesham (55), and Germane (85) obtained little or no evidence that learning accrues from the writing of an essay examination. Stroud and Sones (212) found that, when administered within three days after a learning exercise, the multiple-choice examination had greater learning value than a rereading of the material within the same three-day period. Others have demonstrated the presence of learning in taking tests of the same kind, especially Spitzer (207).

Of special interest in view of its emphasis upon recall is the learning value of the completion test. One limited and certainly inconclusive investigation conducted under the writer's supervision suggests that it is comparatively small (100). The subjects were

college students, the material, a fairly difficult article of about 4000 words. With a shorter article or easier material, or with the same article better learned, more decisive results might have been obtained. (115, 161, 162)

*Some Psychological Aspects.* Tests requiring recall appear to the student to be more difficult than those requiring recognition. Some difficulty may be experienced in convincing a student that a score of 60% right on a true-false examination represents very little proficiency, while no such problem arises with respect to a score of 10% right on a completion test. The effect of correction for "guessing" is, of course, to lower all scores—the low scores more than the high ones—and therefore to increase the range. Correction formulae affect reliability and validity very, very little, and exert an equally negligible effect upon the letter grades of pupils. However, in view of the general lowering of the scores and the increasing of the range, the use of the procedure may "impress" the students, if, of course, the scores are made available. Although they are told that correction makes no difference in the grades they receive, the psychological reasoning is, as Andrew and Bird (4) point out, likely to be more convincing than the statistical. (16, 141)

*Frequency of Examination.* Other things being equal, frequent examinations should be more productive than infrequent ones, even if the pupils respond in the aggregate to the same number of questions. This we may deduce from the fact that the learning or review value of a test is inversely related to the time elapsing between learning and the taking of the test. Rather modest gains from the use of frequent, as opposed to infrequent, examinations have been demonstrated by Turney (229), Keys (128), and Kulp (135). (74, 103, 142)

*The Final Examination.* The relative effectiveness of the final examination will depend upon the use that is made of it and the procedures with which it is compared. As opposed to no method of reckoning, it is certainly recommendable, as investigations by Schutte (193) and Scott (194) have shown. Some plan of exemption might well be as effective as a practice of exacting a final examination of all—especially so, if any considerable number have a reasonable expectation of earning exemption. Remmers (183) found very little difference between the use of the final examination and a plan which exempted about one-third, in the case of several hundred university students.

### IMPROVABILITY IN READING

A review of the experiments in reading is quite beyond the scope of this paper. However, inasmuch as ability to read is fundamental to learning in school, something may be said here about the investigations that pertain to improvement in reading upon the part of those who have reached reading maturity. The following represents a not uncommon procedure: A battery of reading tests is given to entering college freshmen; those who score below a certain low critical point are given instruction in reading one or two hours per week for about four weeks, whereupon the battery is readministered; those who fail to equal or exceed the critical score are given a second period of instruction and, at the end, a third administration of the battery of tests, Forms B or C being used. By this time a large percentage of the students have exceeded the critical score, and this figure is given as the number improved. A statistician would require no better example of the regression effect. If the tests were repeated often enough without any training, nearly all students would ultimately pass the critical score. Thus it becomes comprehensible how a few hours of training can "correct" reading habits that have been formed through years of daily practice!

In an investigation free from such defects Traxler (226) was able to effect an improvement of about 10% in a class of average seventh-grade pupils. Training was carried on intensively and systematically each school day for a semester. McCarty's (146) findings with high school students are of the same order, as are those of Simon (197). (91, 92, 136, 137, 176, 198, 222)

### TRANSFER

That which does not transfer is educationally worthless, if indeed not a positive encumbrance. Except for past learning one could not learn anything in school: the simplest sentence, spoken or written, would be utterly incomprehensible. If a school subject does not transfer in educationally profitable amounts, it is that subject, not transfer, that is invalidated. If "progressive education" is superior to the brand which it opposes, it is, in part, because it proposes readier media of transfer, not because, as some of its proponents aver, it operates on some other base.

The issue has been confounded with formal discipline, the straw man of education. Mental discipline has been a target of educational theorists for three centuries, at least, and the bull's-eye has been none other than ill-fated Robert Recorde's book on algebra (1557),

entitled the *Whetstone of witte*. Locke ridiculed formal discipline; Herbartian apperception was a specific against it; later English scholars, notably Sir John Adams, opposed it. Even if, as has been alleged, the investigations in transfer eliminated the doctrine of formal discipline, they certainly did not eliminate transfer of training.

*Spelling.* The experimental work in spelling relates to the problem of transfer at least at two points, the spelling of derivatives when the base word is learned, and vice versa, and the use of rules. It is found that pupils in the intermediate grades meet with considerable success in spelling untaught words (11) and that a high correlation exists between ability to spell taught and untaught words. Sifert (196) obtained a coefficient of .94. The part that transfer plays in the spelling of untaught words is, of course, unknown. They may have been encountered, some of them a good many times, in reading. A pupil does not have to spell a new word perfectly to show evidence of transfer. Its presence is indicated if he can learn to spell it more quickly by virtue of having previously learned to spell other words or if he can get some of the letters, both of which conditions undoubtedly obtain.

Horn (107) described a circus, without pronouncing the word, to a group of first-grade pupils who had had training in phonetics, and asked them to write the word that stood for the thing described. He obtained 148 different spellings, some of the most frequent, together with the frequencies, being as follows: *srcus*, 20; *srks*, 10; and *circus*, 6. The unphonetic character of English spelling interferes with the operation of transfer and results in many instances of negative transfer. Horn states that the sound *ēr* in *circus* can be spelled accurately or approximately in 21 different ways, as *ir* in *first*, *or* in *word*, *ur* in *fur*, *our* in *journey*, *ear* in *search*, *oir* in *avoirdupois*, and *olo* in *colonel*.

(1) *Derivatives.* Archer (5) obtained evidence of marked transfer, both positive and negative, in adding prefixes and suffixes after the base words had been studied and in spelling the base words after the derivatives had been studied. Negative transfer occurred in instances where the final *e* is dropped before adding *ing* and also in the spelling of certain base words like *excel* when the derivatives *excelled* and *excelling* had been studied.

Horn (107) maintains that each word should be learned as an independent problem. In an earlier article Horn and Ashbaugh (108) had recommended the teaching of every form of the word. Inasmuch as they make pre-testing the criterion of whether or not a word shall

be studied by a pupil, their recommendation is not at variance with Archer's findings. (31, 145, 149, 190)

(2) *Rules.* The value of teaching rules of spelling has been the subject of discussion at least since the appearance of an article by Cook (40) in 1912. Some years later Horn (106) stated that most of the articles dealing with the subject fallaciously assume that the value of teaching a given rule can be ascertained by determining the number of words covered by it. In addition, he says, it must be shown that the rule can be easily taught, that it will be remembered, and that it will "function in the stress of actual spelling."

Watson (235) has demonstrated some efficacy in teaching rules in the case of two small groups of ninth- and tenth-grade pupils. Gates (78) studied the effect of grouping words according to some common element, together with the giving of the rules (Grades 2 to 8). This program facilitated the learning of new words—the only demonstrated value. (129)

(3) *Grouping.* Transfer is implicated in the problem of grouping according to like elements, to the extent that grouping promotes generalization. The following methods of grouping have been proposed: presenting together words of (a) similar meaning, (b) similar sound but of different spelling (homonyms), (c) similar appearance, (d) similar phonetic character (Gates, 78). Somewhat scattered evidence seems to favor grouping, although authorities are not agreed upon its value. (18, 98, 170, 225, 231, 235)

*Arithmetic.* Any generalized skill like arithmetic, reading, writing, and speaking necessarily transfers to immediate situations. Transfer is the *sine qua non* of generalization. A pupil who has learned to subtract can subtract marbles, coins, ducks, soldiers, or whatever circumstance requires. Educators have been concerned especially with the question of the necessity or desirability of teaching all of the addition and subtraction facts. If a pupil has learned  $4+0$ , can he do  $8+0$ ? Does the learning of  $10+6$  or  $20+3$  enable the pupil to respond correctly to  $20+6$ ? Does the learning of  $9+4$  lead to the resolution of  $4+9$ ?

Knight and Setzafandt (133), with pupils just ready to learn to add fractions, engaged one group in adding lists in which 18 different numbers ranging from 2 to 30 were used as denominators. A second group received the same amount of drill in the aggregate, but with the exception that eight of the integers did not appear as denominators in their exercises. The latter group gained about as much proficiency in fractions involving the eight unpracticed numbers as denominators as did the former, which received practice in them. In this situation training transferred almost 100%.

Beito and Brueckner (15) found that pupils (Grade 2B) who were given practice in addition facts in one form only, as  $\begin{array}{r} 7 \\ + 8 \\ \hline \end{array}$ , the larger

$\begin{array}{r} +2 \\ +5 \\ \hline \end{array}$

number always being on top, were quite as successful in adding them in the reverse as in the original form.

Olander (164) compared the scores on tests over the 200 addition and subtraction facts earned by a group of pupils (Grade 2B) who had spent a semester studying all of the facts with those of a comparable group who had devoted the semester to the study of only 110 facts. The performance of the latter was quite as good, in fact slightly better, than that of the group that had studied the full 200. Such investigations do not, of course, demonstrate that there is any economy in not teaching all of the combinations.

Investigations have also been made of the effect of training in generalization. Olander (164) investigated the effect of devoting the first 3 minutes of each 20-minute period throughout a semester to the giving of instruction in generalization. His groups that were taught by the method of generalization plus drill did not prove to be reliably superior in ability to transfer to those that were taught by the method of drill alone. Overman (166) found that various types of instruction in generalization facilitated transfer. (37, 67, 97, 117, 147, 150, 208, 249)

*Transfer in Relation to Brightness.* Like the general investigations of Thorndike and his students (25, 221) and the more recent one of Ryans (187), those of the order described above have shown transfer to be positively related to brightness. Overman (166) reports that the generalization and rationalization procedures used in his investigations in arithmetic were most effective with the highest third of the group in MA. Carroll's (31) study of generalization in spelling yields results of the same order, as does also that of Pratt (175) with respect to the use of algebra in working arithmetic problems. (22, 123)

*Special Training and Improvement Involving Transfer.* Under this heading may be listed investigations like Johnson's (116) on the effect of training in logical thinking on ability to prove theorems in geometry; Stone's (211) on reasoning in arithmetic; Barlow's (12) on training in reasoning; Gilbert's (86) on the effect of reading on spelling; Salisbury's (188) on the effects of training in logical organization; Simpson's (198) on the effect of training in ability to read historical materials; White's (240) on habits of thought; Wagner and Strabel's (232), Eckert and Jones's (56), Behrens' (14), and Edmiston's (57) on how to study; and Corey's (42) on instruction in note taking. (2, 52, 76, 245, 246, 247)

*Language.* Two studies, those of Klopp (132) and Crawford and Royer (46), have compared the relative effectiveness of drill and the formal study of grammar. The former obtained a small advantage in favor of drill, both in formal and in applied grammar; the latter found the two contrasting procedures to possess equal merit. Investigations like the following have treated the effect of one language upon another: Latin on the ability to read English, Thorndike (220); Latin on English vocabulary, Thorndike and Ruger (223), Hamblen (96), Carr (30),

and Otis (165); Latin on the quality of English, Wilcox (243), Miller and Briggs (154), and Woodring (248); modern foreign language on English, Werner (239); Latin on English spelling, Coxe (44); Latin on modern foreign language, Cole (38) and Kirby (130).

### MOTIVATION

Easily the most talked-of law of learning in educational psychology is *effect*. Notwithstanding the fact that its importance in education is almost universally admitted, few proposals for implementing it effectively have been sufficiently well defined to permit of experimentation. The investigations are classifiable under three headings: praise and reproof, knowledge of results, and competition.

*Praise and Reproof.* The studies of praise and reproof, like those of reward and punishment in animal psychology, scarcely permit generalization. No work has shown that one kind of incentive, *qua* kind, is superior to another. Indeed, such could not be shown without the equation as to intensity of the kinds compared. Even in a practical sense, it cannot be said that praise in general is more effective than reproof, or vice versa. The finding that a specimen of praise is superior to a specimen of reproof does not preclude the possibility of obtaining the reverse for some other specimens. Cited here are studies by Gilchrist (87), Hurlock (110, 111), Gates and Rissland (81), Warden and Cohen (233), Brenner (19), Forlano and Axelrod (71), and Forlano (70).

*Knowledge of Results.* The earlier work of Judd (122), and Spencer's (205) repetition of it, Arps (6), and the later work of Book and Norvell (17) gave promise of a consequential yield in education, especially in view of the fact that developments in educational measurement made possible more accurate appraisal of pupil achievement. This promise has at best been only partially fulfilled. In the laboratory it is possible so to arrange an experiment, as that of Judd, that learning is virtually impossible without knowledge of results. This condition does not obtain in school. Moreover, the pupil usually has considerable knowledge of his achievement whether he is informed or not. Also, it is probable that the subjects in the laboratory are more highly motivated than are the pupils in the daily routine of the classroom. Knowledge of results probably functions in guiding the learner in his subsequent performance, if the task is of such a nature as to permit of it, and in mediating pleasurable or unpleasurable effects. To the extent that he does not care, he is not "punished" or "rewarded." In fact, the operation of the law of

effect in school or elsewhere presupposes a motivated subject. Success or failure can produce an important effect only in the event the subject cares, as may be deduced from Ach's laws of feeling. Listed in this paper are the studies of Chapman and Feder (35), Deputy (48), Morgan and Morgan (156), Panlasigui and Knight (168), Brown (24), Ross (185), Fay (68), and Sand (189).

*Competition.* The reader is referred, especially, to the work of the following: Hurlock (112), Myers (159), Sims (199), Sullivan (213), and Zubin (256). Within recent years a good deal of attention has been paid to conditions of motivation in the teaching of various school subjects. For the most part motivation has been only one of several variables. For an account of this work the reader is referred to a recent book of Reed (181); also of interest is Kirkendall's (131) article.

*Miscellaneous.* Under this caption are listed articles by Symonds and Chase (215) on practice and motivation, Gates, *et al.* (79) on systematic and opportunistic methods of teaching, Gates and Graham (80) on the value of various games and activities in teaching spelling, Frandsen (72) on affective tone in spelling, and Shaffer (195) on the influence of style of writing on learning social studies.

#### RETENTION

*The Temporal Rate of Forgetting.* The impression is sometimes gained from the literature that the temporal rate of forgetting is highly variable and that the Ebbinghaus curve is typical of only a narrow range of material, and then only when measured in certain ways. This opinion seems to emanate, at times, from a confusion between *rate* and *amount*. The Ebbinghaus phenomenon is to the effect that the *rate* of forgetting is negatively accelerated, not that a given amount is forgotten within a certain period.

In the best procedure employed in these investigations various objective tests are administered at the beginning of the course in order to assess the amount of previously acquired knowledge. The tests are repeated at the end of the semester; the difference between the initial and final scores is taken as evidence of amount learned. The students are then divided into three or more comparable groups, one of which may repeat the tests after 4 months, another after 8, another after 16, and so on. Almost invariably, typical Ebbinghaus curves have been obtained. Reference is made here to the work of Bassett (13) and Brooks and Bassett (23) in history; Ceder-

strom (33) and Greene (94) in zoology; Eurich (65), Greene (94), Jones (120), and Watson (236) in psychology; Greene (94) in physics and chemistry; and Johnson (118, 119) in botany.

In such investigations the initial and presumably the most rapid period of forgetting has already passed at the time the study of retention begins. Better adapted are the investigations of Dietze and Jones (51) and Spitzer (207). Their subjects studied under controlled conditions certain prose selections and were tested immediately afterward. The tests were readministered once each to various groups after varying intervals up to 100 days (Dietze and Jones) and 62 days (Spitzer). The Ebbinghaus phenomenon obtained. (63)

The majority of the workers have been concerned primarily with the amount, rather than the temporal rate, of forgetting of school subjects. This is especially true of the following: Anderson and Jordan (3), Douglass (53), Eikenberry (59), Frutchey (73), Kennedy (127), Layton (138), Powers (174), Tyler (230), Worcester (252), and Word and Davis (253). On the whole, the pupils have given rather good account of themselves, at least in comparison with the naive assumptions that are sometimes made about forgetting on the basis of Ebbinghaus' results and those of like character. Even so, such studies are poorly adapted to the testing of retention of what is learned in school, especially so if the results are taken as evidence of the value of schooling. Tests that are much more functional in character would give a truer picture of the value of the course, if, indeed, not a better assessment of retention. Transfer of training data could be made to serve as functional measures of retention.

*Retention During the Summer Vacation.* (1) *Reading.* Brueckner and Distad (26), in the Minneapolis public schools, found no difference between the June and September reading scores of first-grade pupils, except in the case of some of the duller groups. Irmina (113) obtained evidence of a slight loss in the first three grades, which was regained by October. Packer and Anderson (167) obtained evidence of a slight loss in oral reading ability during the summer vacation. Morrison (158) reports a gain in ability for the first three grades.

Perhaps, as the foregoing suggests, there is a slight loss during the summer vacation on the part of the primary grades. The studies at the intermediate grade level give evidence of a slight gain, taken as a whole. Bruene (27), Morgan (157), Schrepel and Laslett (191), and Elder (60) obtained evidence of a gain; Noonan (163), no change; and Patterson and Rensselaer (169), a slight loss.

(2) *Spelling.* Investigations show without exception a loss in spelling ability during the summer vacation, the lower grades losing more and requiring a longer time to regain the loss after the resumption of school. (7, 27, 113, 160)

(3) *Arithmetic.* Some workers have obtained evidence of a loss in arithmetic amounting to as much as half of the previous year's gain; others report that the June level is not regained until December. Practically all have shown some loss. On the whole, computational skill deteriorates more than arithmetic reasoning. (27, 75, 113, 157, 158, 160, 191)

(4) *Content Subjects.* Morrison (158) observed a gain in history scores in the intermediate grades; Irmina (113), a gain in history and literature; Bruene (27), a gain in nature study and a loss in history and literature. At the junior high school level Brooks and Bassett (23) found that the scores in American history, after a lapse of four months, were about 80% of their level at the end of the school year. At the college level, Johnson (118, 119) obtained a loss of about 40% in scores in botany; Greene (94) reports decreases amounting to from a third to a half in zoology, psychology, chemistry, and physics.

(5) *Typing.* A single study by Schroeder (192) on a group of 20 high school students in first-semester typing found no average loss in skill during the vacation. It is stated that none of the students had practiced during the summer.

*Relation Between Immediate and Delayed Recall.* Eurich (65) obtained a coefficient of .59 between immediate scores and those obtained nine months later in Psychology I; of .66 between immediate scores (in March) and delayed scores (in October) in Psychology II. Johnson (118, 119) obtained a coefficient of .75 between scores earned at the end of a course in botany and scores on the same tests readministered six to nine months later, for one group, and of .84 between immediate scores and those obtained three to nine months later, with another group.

The correlation between immediate and delayed scores decreases as the interval between the two testings increases, as the article by Dietze and Jones (51) shows. They obtained an *r* of .75 between immediate and 1-day scores, of .45 between immediate and 100-day scores, and of .35 between 1-day and 100-day scores. Reliability coefficients of tests also decrease as the time between learning and testing increases—a fact that could well account for Dietze and Jones's findings. Dietze (50) obtained an *r* of .55 between immediate scores and MA and an *r* of .25 between delayed scores (100 days) and MA.

*Effect of Review.* In educational psychology the temporal position of review has been a subject of discussion since Lyon's (144) pronouncement on the subject in 1914. Lyon recommended that material be reviewed soon after it is learned and that of several reviews the initial ones be relatively frequent. From the context it appears that Lyon had in mind symbolical review, recall.

The writer has come across no research, save that of Lyon, that is

specific to the problem. Related data support Lyon. Krueger (203) found that recall, as a method of learning, is of little value in the initial stages. Perhaps this finding is an instance of a more general condition—namely, that the efficacy of recall is a function of the strength of the connections. There is some logic in assuming that in review the value of recall should decrease as the interval between learning and recall increases—that is, as the strength of the connections decreases. Perhaps the closest approximation to a test of Lyon's recommendation is Spitzer's (207) finding that the review value of taking an objective test decreases as the interval between learning and testing increases.

There is another kind of review, reimpression. There is no indication that the value of this kind of review decreases as the time between learning and reviewing increases. Bridge (20) found no difference as between engaging in various summaries at the end of a class period and at the beginning of the lesson on the following day. Peterson, *et al.* (172) found review, by rereading, to be as effective on the seventh day as on the second. Stroud and Sones (212) compared the relative effectiveness of two kinds of review, (a) by rereading and (b) by multiple-choice test response, at three temporal positions: I, on the first and second days after learning; II, on the eighth and fifteenth days; and III, on the fifteenth and seventeenth days, the criterion test being administered 42 days following the original study period. Review by test response proved to be reliably superior to rereading at the first position. The value of the test response decreased at each of the other two positions; that of the rereading review did not decrease. At the third position rereading was reliably superior to the test response. (55, 104, 178, 200)

#### LABORATORY AND SCHOOL EXPERIMENTS

In going over the literature the writer has gained the impression that the discrepancies between the experiments of the laboratory and those conducted in school situations are not insufferable by any means. He has a feeling also that differences in the materials employed in the two fields of research are not so serious as those involving the use that is made of them. The fact that lists learned in the laboratory are usually made up of nonsense syllables does not present so great an obstacle to applicability in school as does the fact that the learning activity is memorization.

One naturally inquires: "How good is the experimental work on learning in school?" On the whole it compares not unfavorably

with the work done in the laboratory. Probably none of it is so good as the very best work of the laboratory. The future holds considerable promise, especially if experimental psychologists will in greater numbers familiarize themselves with educational problems and feasible practices, and if educational psychologists will give more attention to the best experimental procedures. Closer coördination is recommended.

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## MATHEMATICO-DEDUCTIVE THEORY OF ROTÉ LEARNING

### I

HULL, C. L., HOVLAND, C. I., ROSS, R. T., HALL, M., PERKINS, D. T., & FITCH, F. B. *Mathematico-deductive theory of rote learning: a study in scientific methodology.* New Haven: Yale Univ. Press, 1940. Pp. xii+329.

### PART I. THE PSYCHOLOGICAL SYSTEM

The fifty-five years since Ebbinghaus' monograph appeared have seen countless experiments performed within the nonsense-syllable tradition which it inaugurated, but the contributions to psychological theory arising out of the systematizing of the data from these experiments have been relatively few. In this monograph by Hull and his collaborators we have the most extensive and impressive handling of rote learning since Ebbinghaus first fitted a logarithmic curve to his data. Indeed, it is without doubt the most detailed systematic treatment of any field of behavior up to the present time, not excluding the work on psychophysics and factor analysis. The system is Hull's. He has been assisted in its elaboration by the mathematics of Ross, Hall, and Perkins, and by the logic of Fitch. To Hovland goes the credit for discovering and assembling existing experimental data confirming or negating the theoretical deductions throughout the book, and for providing many new data in experiments designed specifically to test some of the deductions.

The formal problem is an extension and refinement of the miniature system earlier proposed by Hull (*Psychol. Rev.*, 1935, 42, 491-516), in which he first adopted the method of definition, postulate, and theorem, there described as the "geometric" method. In its extension more use has been made of ordinary mathematics (*i.e.* algebra, the calculus), and symbolic logic has been introduced for a more rigorous formulation of the definitions and postulates. As a study in methodology it is proposed to show that a system can be developed for the quantitative handling of representative psychological data whereby, with a limited number of self-consistent definitions and postulates, numerous verifiable experimental relationships can be derived as theorems by logico-mathematical processes. Correspondingly, where contradictions appear, or where the deductions do not

square with data, the system exposes its own weaknesses, so that in successive approximations better systems may emerge.

The content of the system is best summarized by reference to the theorems, the derivation of which (with their corollaries) constitutes the bulk of the book. Because of the method adopted, topics appear where they naturally emerge as theorems or corollaries, or where they are brought in to provide the articulation for a proof. As a consequence of this logico-mathematical structure, the book is somewhat lacking in topical structure. That is, there are no chapter headings about the course of memorization, the curve of retention, the effects

TABLE I

MAIN TOPICS CONSIDERED, THE THEOREMS DEALING WITH THEM, AND THE POSTULATES CHIEFLY USED IN THEIR DEDUCTION

Topic	Theorems	Essential Postulates
(1) <i>Serial Position Effects</i> Various relationships having to do with the effects of the ordinal position of a nonsense syllable in the list being memorized or recalled.. .	1-9, 16-17, 20-34, 54	1-11
(2) <i>Reminiscence and Forgetting</i> The initial rise in the curve of retention shortly after memorization and the subsequent decline..... (a) As related to the distribution of practice .....	10-11, 35-44	12-13
(b) As related to serial position.. . (c) As related to length of list... .	12-15, 54 45-50 51-53	
(3) <i>Reaction Thresholds and the Course of Memorization</i> The separation between the first successful recitation of a given syllable and the last failure before mastery; the increasing probability of success with repetition.....	16-19	14-17

of length of list, and other such familiar topics in the literature of rote learning. To provide an overview of content, the reviewer has prepared Table I, which, although oversimplified and condensed, points to the nature of the topics covered by the theorems in relation to the postulates used in their derivation.

Some looseness of grouping arises in the table because the interrelationships cannot easily be summarized in such a brief listing. The only postulate not mentioned in the table is the final one (18), which has to do with latency of reaction. It permits a number of theorems and corollaries depending upon the correlation between latency and success of response, *i.e.* the more probable a response, the lower its

latency. Nearly every statement with respect to probability of response can be mirrored with a corresponding one of latency.

This impressive body of material, 54 theorems, 110 corollaries, and 8 problems, a total of 172 specific deductions, is harmonized by the 18 postulates and their underlying definitions. The claim to validity as a system rests in part on the economy which is achieved through deducing a wide range of phenomena from a relatively compact set of principles. How this harmonization has been achieved may be suggested by considering the relationship of sets of postulates to the major topics of Table I.

There are essentially three postulate-theorem systems, having to do with serial position effects, the course of retention, and the course of acquisition, respectively. There are, of course, a number of interrelationships between and among these subsystems, as shown most strikingly by the appearance of the same constants in more than one of them. For example, the mean reaction threshold, expressed by the constant  $L$ , appears in the equations for serial position effects as well as in those concerned with the course of memorization. The broad outlines of the derivations are more easily followed, however, by considering the three subsystems separately.

*Serial Position Effects.* In the earlier 1935 system, serial position effects were derived on the basis of inhibition of delay, on the assumption that the recall of items near the middle of the list was inhibited by the remote associations spanning these items. While this derivation was very neat, based as it was on conditioning principles applied in a new setting, it was recognized at the time that the derivation was too simple, since maximum difficulty was deduced to be at the middle of the list, whereas it actually falls somewhat beyond the middle. In the new system, the fit of the theoretical curve to the empirical one is achieved for lists of different length by an elaborate mathematical manipulation of the consequences of eleven postulates. These postulates refer to the stimulus trace which is said to persist after the exposure of a nonsense syllable, and to the excitatory and inhibitory potentials which are generated following each repetition of a list. An excitatory potential refers to an increased probability of response; inhibitory potentials decrease this probability. While the adjectives 'excitatory' and 'inhibitory' bear some of the flavor of their origin in conditioning literature, the liaison between the postulates and conditioning in the new system is extremely tenuous. The precision of the achievement, which is a masterpiece of mathematical ingenuity, has required postulates with respect to the behavior of excitation and

inhibition which appear to be *ad hoc* inferences. According to the rules of theory construction, one is free to postulate anything coherent with the definitions, but these postulates (particularly Nos. 4 to 7) appear to be the least likely to prove of service in other contexts. The authors have themselves acknowledged this difficulty quite explicitly (p. 307). One must be fair to the methodology, however, and accept the fact that these postulates have made it possible to formulate in mathematical terms a fair approximation to the effect of serial position. Until someone does as well with other postulates, it is scarcely legitimate to object to the particular postulates chosen.

What is meant by a 'good fit' when a theoretical curve is superimposed on an empirical one is, of course, a relative matter, and the authors are in this respect their own most severe critics. From their point of view, the derived curve for serial position effects does not fit very well, particularly for the early syllable positions. They are dissatisfied also because the constants derived through the process of fitting curves to data from lists of different length do not agree, although circumstances of experimentation were held quite constant except for length of list. An important criterion of a good scientific system is that it should have a few constants, empirically derived, reappearing in various contexts, like the gravitational constants or the speed of light in the equations of physics. The authors have gone about the task of providing appropriate constants, such as the threshold of recall ( $L$ ), and its standard deviation ( $\sigma$ ), the unit inhibitory potential ( $\Delta K$ ), the reduction factor ( $F$ ), all of which should reappear at the same (or systematically related) values in different deductions. While by their own testimony the authors have not succeeded very well in determining the numerical values of these constants from the data at hand, the attempt to secure such values is noteworthy as an effort at sound system construction.

*Reminiscence and Forgetting.* The second subsystem rests primarily on postulates 12 and 13, which refer to the decay with time of both excitatory and inhibitory potentials. By choosing appropriate parameters, the interaction of the two curves of forgetting yields the reminiscence function described by Ward. Since the advantages of distributed practice over massed practice are attributed to reminiscence over the rest intervals, the theorems with respect to massed and distributed practice depend primarily upon these same postulates.

The empirical agreement with Ward's data is practically a matter of curve fitting, for his curve is derived by first factoring it into two others, then adding them together. Having selected the fitting func-

tions, however, their further implications are carefully studied. Agreements and disagreements with empirical findings are carefully pointed out. Through an oversight of some sort, one improper use of analogy was made in the choice of evidence to support one of the theorems with respect to distributed practice. Although the advantage of distributed practice is supposed to lie in reminiscence over the intertrial interval, which is at its maximum between the second and third minute, confirmatory evidence is used from Lyon (p. 131), although his data were secured with trials spaced at one per day. Any advantage over such a long rest contradicts the very theorem the data are used to support. The postulates account very well for what happens in the first twenty minutes after learning (within which time limits the theorems with regard to forgetting are, in fact, constrained), but the postulates do not serve for things that happen overnight or that happen when trials are spaced by more than a few minutes. The authors recognize the difficulty later by pointing out that the facts of Jost's Law are contradicted by the system (p. 307). The basic trouble is that forgetting of excitation is described as continuous decay, and no provision is made for the rapidity of relearning and for the longer retention of repeatedly learned material.

*Reaction Thresholds and the Course of Memorization.* The third subsystem is based on postulates 14 to 17, which have to do with reaction thresholds and their variability. These postulates account for the fact that a syllable correctly recited on one trial may not be recited on the next, although the probability of correct recall increases with practice. Some of the most novel relationships within the empirical data of rote learning appearing in the monograph have been discovered as a result of the theorems ingeniously derived with the aid of these postulates. Contributions here include novel methods of scoring rote learning, such as those arising out of concern over the separation between the trial at which the first success occurs with a particular syllable and the one at which the last failure occurs, and novel methods of plotting the course of memorization, so that an ogival learning curve is secured. These sections (pp. 146-171) would repay study by anyone interested in the empirical aspects of the memorization problem, whether or not he wished to make the effort to master the systematic exposition of other parts of the monograph.

It is pointed out that the learning curve, predicted to be symmetrical around an inflection point, is actually somewhat skewed, so that revision is needed (p. 164). The lack of agreement is not fundamental, however, and the deductions are essentially successful.

This is not an easy book to read: like advanced works on psychophysics and factor analysis, it is only for those both willing and able. To make it somewhat easier for those with different technical training to follow the argument, the steps are treated in sections labeled A, A', B, and C. The A-sections contain the formal statements of the postulates, theorems, and corollaries, with the mathematical proofs. The A'-sections, used only with the postulates, restate them in terms of symbolic logic. The B-sections give a common-sense statement of the argument, using ordinary verbal language and relatively simple arithmetical illustrations. The C-sections, used chiefly with the corollaries, cite experimental evidence bearing on the empirical validity of the propositions presented in the A-sections.

A novel and highly important aspect of the conscientious effort at objectivity represented by the monograph lies in the careful summaries of evidence in the C-sections of the corollaries, pointing out agreements and disagreements between the deductions and the data from experiments. Unlike many systematists within psychology, who select data to fit their theories and rationalize away objections, these authors have been as careful to expose the weaknesses of the system as to advertise its strong points. Evidence is available bearing on 71 of the 110 corollaries. The evidence clearly supports the corollary in 39 cases (55%), disagrees sharply with the corollary in 12 cases (17%), and is ambiguous in the remaining 20 cases (28%). Less rigorous standards of judging agreement and disagreement might raise the score for successes above that stated, for the ambiguous cases point preponderantly in a favorable direction. At this early stage of system-making, too high a proportion of success would lead to the suspicion that results were worked backwards from data rather than forwards to new deductions. While there is no logical objection to working backwards, since ultimately that is the only way in which to correct a system, it is evidence of a genuinely fertile deductive method that so many potentially verifiable propositions have been stated for which experimental evidence is not now at hand. It is useful to present the corollaries which are known to disagree with experimental findings, for improving the system requires that its weaknesses be exposed as clearly as possible so that the defects may be diagnosed accurately.

While the steps in derivation have been taken with great care, a few inaccuracies, not inherent in the system, have inevitably crept into the text. A few statements have undergone unpermissible changes as the exposition proceeds. Thus, excitatory and inhibitory potentials which have been defined in such a manner as to be limited

to the *first* massed practice (D64, p. 36; D71, p. 38) are later used for *any* massed practice (pp. 60 ff.). The copying of elaborate mathematical formulas has produced a few errors which make it difficult to follow specific derivations, but these are chiefly typographical and are easily cleared up. As an example may be mentioned an equation, repeated twice on page 77, in which the left-hand side is identical with the numerator on the right-hand side, thus forcing the denominator on the right to be unity, which under the circumstances cannot be the case. The correct form of the equation may be found on the preceding page, however, so that no harm is done. Rather than censure the authors for permitting such an error, they are to be commended on the relative freedom from error in a typographical task of this magnitude.

Having seen the demonstration that a system of this kind can be prepared, the question arises: Is this the natural trend for psychology to take? If the answer is in the affirmative, it means that psychologists must learn more mathematics and logic than they have in the past. The day for mere fact-gathering is undoubtedly over; it is certainly desirable for psychology to become deductive. The only patterns of advanced sciences which we have suggest that this requires mathematical precision, with interchangeable constants related by equations in which variables are defined, for which units of measurement exist, and so on. If this is indeed our pattern, the monograph is a pioneering effort in the right direction.

There remains the possibility that it may not yet be efficient to attempt to elaborate many systems as detailed as this until more foundational work has been done. Without rich content we cannot have a satisfactory system, no matter how perfect its formal aspects. The success of the present system rests upon the laboratory study of rote learning through half a century. Even so, its very formality makes it difficult to include many well-established facts within the field of memorization and retention: the matter of instructions, the desire to learn, meaningfulness of materials, similarity, familiarity with items, effects of pleasantness and unpleasantness, retention of finished and unfinished tasks, the phenomena of retroactive inhibition. This is, to be sure, partly a matter of size of task rather than of logical impossibility, for there are in the monograph seven additional theorems concerned with anticipatory errors (p. 304) for which proofs are not given simply because the book is already so long. It must be recognized that foundational work is often best directed by having a system to pose the questions. Testimony to this is found

in the new data on rote learning in the monograph which mere blind search would not have uncovered. Probably the best answer with respect to the place of this kind of system in psychology is that it is never too early to begin, and the more efforts as careful as this one by Hull and his collaborators, the better for psychological science.

The book gives a foretaste of what psychology will be like when it reaches systematic, quantitative precision, without its system necessarily being a part of that future psychology. Science advances by approximations and gradually becomes transformed. This book will take its place among psychology's transforming approximations.

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## PART II. THE LOGICAL SYSTEM

The Mathematico-Deductive Theory of Rote Learning is presented in two versions, which, like the two versions of the Covenant of the League of Nations, are translations of one another. In the one the definitions, postulates, theorems, and the demonstrations of the theorems are written in English with the help of mathematics, and the rigor of the demonstrations is the rigor of ordinary proof. In the other they are written in the notation of symbolic logic, and the rigor of the demonstrations is the rigor of symbolic proof. For reasons which are obvious to the logician, the second version is presented only *in potentia*. With one exception, none of the theorems are formulated in this notation, and none are deduced from the postulates by symbolic methods. But Mr. F. B. Fitch, who executed the difficult task of formulating the definitions and postulates in the notation of *Principia Mathematica*, has undoubtedly satisfied himself that all the theorems without exception are derivable from the postulates when they too are expressed in this notation.

The reader who is innocent of symbolic logic will object that the presentation of the same subject matter in two languages, with one of which he is thoroughly unfamiliar, is both useless and futile. It would be ridiculous, he will say, to waste one's time learning French merely in order to read the French version of the Covenant, when an English version is available. What is more, if the two versions are accurate translations of one another, the rigor of symbolic proof is the mirror image of ordinary proof, and symbolic proofs can therefore not increase our confidence in the validity of the theorems.

Mr. Fitch has answers to both of these objections. The first is answered by pointing out that the formulation of a scientific hypothesis in the notation of symbolic logic "will generally serve to elucidate what is really intended to be assumed," and that, furthermore, "the careful analysis of a hypothesis by symbolic logic often reveals subtly different (and perhaps better) hypotheses which never would have been imagined had only ordinary language been used" (pp. 309-310). The second objection is answered by appealing to the superior rigor of symbolic proof: "When it once becomes clear what is being assumed, then it also becomes clear whether a given proof of a theorem is really valid or not" (p. 309).

These are excellent reasons for the exposition of a deductive system in the notation of symbolic logic while the system is under construction. They do not answer the objections of the reader. The reader argues as follows: "When I want to build a house I engage the services of a competent architect. I commission him to draw the plans for the kind of house I want to build; I do not find it necessary to become proficient in architecture myself in order to build houses. And I certainly do not permit the architect to bring his colleagues and clients into my house after its completion. If he needs a forum for his architectural disquisition on its construction he can go elsewhere. When the house is finished I dismiss him and discard his plans. Now substitute 'deductive system' for 'house,' 'logician' for 'architect,' 'symbolic logic' for 'architecture,' make the other requisite substitutions, and you will understand my conception of the relation of the logician and of symbolic logic to deductive systems and to their construction." The fact that Mr. Fitch's logical talents kept confusion and ambiguity away from the formulation of the definitions and the postulates, that they often revealed different or better postulates, and that they matched the ordinary proof of every theorem by a symbolic proof, only shows that symbolic logic can be of invaluable help in the construction of a deductive system and that it can guarantee the soundness of the construction. It does not show that its analytical formulations and proofs cannot safely be removed once the construction is completed. The scaffolding is indispensable while the building is under construction; the finished building does not require it.

These remarks are not intended to deprecate the publication of Mr. Fitch's ingenious contribution as part of the monograph. The purpose of publishing Mr. Fitch's pioneering, I take it, was to show psychologists what services a logician can render in the construction

of a deductive system and not to convince them that the time has come when every psychologist must acquire proficiency in symbolic logic. But, alas, psychologists will not properly appreciate these services without learning symbolic logic.

Any psychologist who desires to follow the trail blazed by Mr. Fitch can equip himself for the enterprise in a course of study such as Mr. Fitch recommends on page 309. He should be cautioned, however, that his ability to follow a trail will not at once qualify him to do his own trailblazing. The translation of a deductive system into the language of symbolic logic is not a problem that can be left to the amateur. The aspiring psychologist should also be warned that the road that leads from the postulates to the theorems is not a royal road; symbolic proofs of theorems are generally long, devious, and arduous. A comparison of the symbolic and the ordinary proofs of Corollary 1 of Postulate 1 will give him an idea, though an inadequate one, of the lengths to which a symbolic proof may go. Mr. Fitch's symbolic proof, though greatly abbreviated, requires almost three pages, with approximately thirty lines to the page, for its presentation. The ordinary proof is printed on a single line.

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## II

### SYMBOLIC LOGIC AND BEHAVIOR THEORY: A REPLY

BY FREDERIC B. FITCH

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In Dr. Paul Marhenke's review of the symbolic logic portion of *A mathematico-deductive theory of rote learning* he expresses certain views which seem to indicate that he has underestimated the potential value of symbolic logic in natural-science theory. He grants that symbolic logic provides a very valuable technique for analyzing and choosing between different possible psychological postulates and for disclosing postulates which might not otherwise be thought of, but he apparently holds that symbolic logic can be safely dispensed with and omitted when the final symbolic formulation of the postulates has been decided on and translated into English. He therefore believes that the final version of any psychological theory can be adequately stated in ordinary language.

Professor Clark Hull and I have come to the conclusion, however, that symbolic logic can never be completely dispensed with in any rigorously formulated psychological theory which is sufficiently complex to be of interest to psychologists. Statements in English can be only rough and partially inaccurate translations of postulates and theorems. The latter can be accurately formulated only in symbolic logic. For example, definitions D2 and D30 in the above-mentioned monograph both use the concept "sequence," but in importantly different ways. This difference would be tedious to explain without the use of symbolic logic, but it is a difference which is at once obvious and clear to anyone who reads the symbolic-logic formulations of D2 and D30. The main point of the difference is that in D2 a sequence is regarded as an irreflexive relation, while in D30 it is regarded as a reflexive relation. If all such distinctions were to be explained in ordinary language alone, the actual psychological theory would become buried in a mass of logical discussion to such an extent as to be utterly unwieldy.

It may be possible to throw away the blueprints of a house after the house is built and suffer no resulting inconvenience; but it is not possible to throw away the symbolic logic used in building a precise psychological theory without throwing away the essence of the theory itself. The English statements of the postulates are, for most purposes, fairly good approximations to the meanings of the corresponding statements in symbolic logic, but in many respects they are liable to be vague and even misleading. Such "deductions" as are made from English statements are always open to slight (and sometimes grave) doubt until formalized in symbolic logic and compared with symbolic formulations of those English statements from which they are supposed to follow.

If the objection is made that mathematicians can often state their postulates in ordinary language and carry out rigorous deductions without resorting to symbolic logic, a twofold answer can be given:

(1) The concepts of mathematics have achieved a clarity which the concepts of none of the empirical sciences possess—least of all, psychology.

(2) It is coming to be recognized even in certain branches of mathematics that the only satisfactory criterion of rigor is translatability into symbolic logic.

Although the clarity of the mathematician's concepts enables him to ignore his criterion once he has used it, the empirical scientist cannot in highly complex psychological situations similarly dispense

with symbolic logic and still claim great deductive rigor. Indeed, the wealth of hidden, implied meaning attached to many commonly used psychological concepts, such as "goal," "response," "tendency," etc., is so great that when such concepts are introduced into English postulates the theorems supposed to follow from such postulates are liable to vary over a wide range of choice, depending upon what meaning is associated with these concepts in virtue of the psychological training (or other background) of the person "deducing" the theorems. In a symbolic-logic formulation no properties (*e.g.* of "goal") can be deduced which do not follow logically from properties which are *explicitly* assumed as belonging to the concepts being dealt with (instead of from properties which are implicitly associated with such concepts in the mind of some individual). Only a formulation in symbolic logic, and appropriate deductions similarly formulated, all retained in the presentation of a psychological theory, can pare off these vaguely and unconsciously assumed conceptual accretions and make possible genuine scientific methodology.

## BOOK REVIEWS

KATONA, G. Organizing and memorizing: studies in the psychology of learning and teaching. New York: Columbia Univ. Press, 1940. Pp. xii+318.

A preface by Wertheimer explains that this book is an application of gestalt principles to the field of learning, in which field pre-gestalt research and theory have been limited to senseless aggregations of material, and to the effects of "and-summation" and of repetition. The historical reasons for this limitation are, he explains, centuries of the use of mere drill as a method of teaching in schools, the mistaken assumption made by certain philosophers and psychologists that learning is essentially a forming of connections between arbitrarily aggregated items, and the search for experiments that can be thoroughly controlled and submitted to quantitative treatment. The methods of gestalt psychology which open the way to features other than "and-summation" and repetition have "led to a re-understanding of what grasping is, what understanding is, and what it requires—in perception, in learning, in other fields of psychology" (p. vi). Katona's book, with its thesis that understanding is the prototype of all learning, is an exposition of this re-understanding and a challenge to the thesis (p. 238) that the basis of learning is mechanical memorization.

Learning is defined in terms of success and improvement. "If a performance is better than a previous performance . . . and if, moreover, the improvement is due to the effects of the previous process, then we call the previous process "learning" (p. 3).

The experiments described in the book were designed to test the assumption that there are two kinds of learning. "Connections established by the conditioned-reflex technique or by repeating the same contents or responses over and over again, as in all forms of drill, are characteristic of one kind of learning. Then we draw a thick dividing line. On the other side of the barrier we find processes of learning that are described by expressions such as 'apprehension of relations,' 'understanding of a procedure,' 'insight into a situation'" (p. 5). The first kind of learning that the associationists have been investigating Katona calls "senseless" and the other kind "meaningful." "Our assumption then," he says, "is that the processes described by these labels differ from each other in the sense that the laws and rules of one do not apply to the other."

A preliminary experiment which will serve to illustrate the nature of the investigation consists in asking three classes of ten adults each to learn the series of twelve digits: 581215192226. One class reads this three times in groups of three digits, as 581 215 192 226. Immediately after reading this three times, nine of the ten are able to recall the series without mistake.

The second class is asked to read three times a card bearing the legend: "The Federal expenditures in the last year amounted to \$5812151922.26." This class did slightly worse than the first on immediate recall; but when five of them were asked one week later to remember the series, the members of the first class were annoyed and no person gave more than five digits correctly, while four out of five members of the second class recalled that expenditures exceeded 5 billion, 810 million.

The third class was asked simply to "try to learn the following series." There were signs of understanding, surprise, and pleasure as they studied the figures. Two members of this class discovered the principle of the series (the alternate addition of 3 and 4) and were able to recall the series perfectly and to repeat this perfect recall a week later. Four weeks later one of these subjects had perfect recall, while another remembered the principle but started with 7 instead of with 5. Another subject began correctly with 5 but used the wrong principle, with intervals of 2 and 3 instead of 3 and 4.

When learning has been accomplished by "finding a principle," retention lasts longer, learning is possible *without any practice of, or experience with, the series*, recall is with greater certainty, reproduction is not necessarily the exact repetition of all items, an altered series can be corrected, and the series can be continued. The connections between items are no longer "senseless" but have an inner necessity.

The elemental difference between senseless and meaningful learning lies in grouping. In Class 1 the grouping is: 581 215 192 226. In Class 2 it is: 5 812 151 922 26. In Class 3 the grouping is: 5 8 12 15 19 22 26. The first is arbitrary; the last is in terms of a principle. Grouping obeys the gestalt laws of perception and is basic to learning by understanding.

Two other types of ingenious experimental material are used. An instance of the first of these is a card trick in which the problem is to arrange four red and four black cards in such an order that these may be dealt alternately face up on the table and at the bottom of the deck, having the tabled cards fall alternately red and black. This may be varied in several ways. The number of the cards may be changed, which alters the order of the solution; or, what is essentially the same problem, even and odd numbers instead of red and black color may be used. The second type of experiment used problems in arranging matches in which, for instance, sixteen matches are placed so as to form five squares, and the problem is to reduce the number of squares to four by changing the position of only three matches. This may be varied by using many different match patterns.

In both types of material the learning is done, according to Katona, in two fundamentally different ways, by direct practice or repetition of the solution and by understanding the problem. As in the case of the digit series the "senseless" learning may result in better immediate recall of the demonstrated solution, but meaningful learning is more stable and results in the "learning" of tasks that have not been practiced. After understanding the principle, the subject is enabled to solve similar problems though these are new to him. Senseless learning is shown to be a process different from learning by understanding. The latter is essentially

problem-solving, the discovery of a principle; and this lies in changing or organizing the material. After such learning, the individual is equipped to meet many new situations that are similar only in certain whole qualities, not in specific detail.

Chapters on *Transfer* and on *Forgetting* and *Repetition* apply gestalt methods to these fields. Here, also, learning by understanding is a process that can be described in terms of the author's thesis that "the process of gradual organization, the slow transition from a worse to a better state of affairs, from a bad to a good gestalt, is just as important for the psychology of meaningful learning as is the flash of insight" (p. 159). In one experiment "each repetition contributed to the crystallization of knowledge and of the geometrical problems. Each consecutive example broadened and deepened the understanding of the principle" (p. 160). "The effect of learning . . . should here be characterized only briefly as a smoother process of greater unity and better organization."

A chapter on *Grouping* and *Learning* develops the rules and the principle of grouping. A chapter on the nature of the memory traces that underlie learning suggests that there are traces of two sorts corresponding to the two sorts of learning. Those underlying specific and "senseless" learning display a certain degree of fixation and rigidity, while structural traces are more readily adaptable and flexible. A "well-centered trace" may develop "by a process of perceptual re-organization or by a crystallization of the traces." A final chapter on *Education Implications* suggests that pupils should not merely learn by memorizing but should "learn to learn by understanding."

If Katona's book encourages psychologists to devote more attention to "learning by understanding," that will be a noteworthy achievement, because there is no doubt that this is a valid descriptive category and is suffering from neglect. But Katona's attack is as notable for what it disregards as for what it takes account of. That grouping is basic in learning by understanding he recognizes; but that grouping itself is a response, and that learning by repetition and learning by understanding represent different activities of the organism, and the possibility that both of these types of activity may conform to the principle of association, he does not appreciate. The reviewer finds no evidence in the book that would contradict the assumption that Katona's subjects learned to respond in the circumstances as they had been led to respond in those circumstances previously. "Discovering a principle" may turn out to be the associative learning of a phrase or a pattern of reaction. The subjects who, according to Katona, have learned not only the series practiced (5 8 12 15 19 22 26) but all other similar series may be found on examination to have learned "add three, then four" as a response to a digit presented under like circumstances. Katona is emphatic in asserting that an explicit verbal phrasing of the principle is not essential and often does not occur. This is, of course, possible, but when the explicit verbalization does occur he appears to regard it as a mere by-product and does not recognize it as the presumptive vehicle of the acquired ability to solve problems.

By defining learning in terms of success, Katona eliminates from observation and from the record the material that should be of greatest interest to the psychologist, the acquisition of habits and behavior that lead to failure. Whether associations are "senseless" or not depends on their outcome and not on the associative process. Avoidance called out by a cue merely because that cue has accompanied injury is not senseless avoidance; it is merely associative avoidance. Its success or failure depends on the world about and lies in another universe of discourse. The assumption that the nervous system displays two types of adjustment to the world, one that leads to success and one that leads to failure, might be defended in the field of ethics or of theology, but not in the field of natural science.

Katona's report of these experiments establishes that under certain circumstances his subjects have achieved certain commendable results, to which he gives complimentary names like "insight," "crystallization of knowledge," "integration," "broadened and deepened understanding," and no one would dispute this. But he has shown no disposition to inquire into the process by which these results are achieved and has certainly not eliminated the possibility that the acquisition of these commendable abilities may prove to involve associative changes in behavior.

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RHINE, J. B., PRATT, J. G., STUART, C. E., SMITH, B. M., & GREENWOOD, J. A. Extra-sensory perception after sixty years. New York: Holt, 1940. Pp. xiv+463.

The purpose of this volume as expressed in the Preface is to "include in this survey everything that is of importance to know in deciding whether ESP occurs, and what it is like if it does occur." The chapter headings suggest that this has been done, since the problem, methods, results, criticisms, and conclusions are included. The problem or hypothesis to be used in testing for the occurrence of ESP is more clearly stated than has been the case in the past, and the chapter discussing statistical methods and mathematical considerations is an excellent summary. However, the rest of the book falls far short of fulfilling its expressed purpose. It would be extremely difficult, if not impossible, to reconstruct a single ESP experiment from the sketchy description of methods even if this should be supplemented by reference to other chapters, including one in which six experiments are tested "in detail" against the counter-hypotheses suggested by critics.

In spite of the fact that five of the eighteen chapters are devoted to a discussion of criticism and counter-hypotheses, the tone of the work is definitely uncritical. One chapter summarizes the criticism of ESP methods in the literature, and another contains the contributed criticisms of three of seven psychologists asked to criticize the chapters on results and counter-hypotheses. All of the criticisms, both those previously published and those contributed, are too easily dismissed by referring

them to the authors' discussion of counter-hypotheses (which was included in the material criticized by the contributing writers).

The chapter in which the counter-hypotheses are refuted individually deserves special mention. The authors summarize this chapter as follows: "In Chapter V, all the counter-hypotheses that have been proposed were presented and tested singly against the available research reports which related to them" (p. 244). If one accepts the counter-hypotheses as fair restatements of the criticisms which they represent, this summary is acceptable. However, the counter-hypotheses are presented in such a form that each applies to *all* of the experiments giving results favorable to ESP. In this vulnerable form they are knocked down one at a time by pointing out one or more experiments in which the mechanism proposed by the counter-hypothesis could not have operated. For example, to refute the sensory cues criticism, experiments involving distance between the subject and the experimenter are cited. No indication is given that the sensory cue criticism is applicable in much of the ESP work not involving distance.

Actually, the majority of the counter-hypotheses were not originally presented as blanket criticisms. Consequently, the authors' method of refuting them by pointing out experiments to which a given criticism does not apply is irrelevant and has no logical value as a refutation.

The authors are on somewhat more sure logical ground in the following chapter, in which they test six experiments against all of the counter-hypotheses jointly. However, the six experiments are summarized in a way that would not permit the reader to judge of their adequacy for himself, and inaccuracies occur in the descriptions which are given. Another writer (Kennedy) has indicated that two of the six might be explained by other than ESP hypotheses, and a third will be criticized in a paper being prepared by the reviewer.

A number of significant omissions should be mentioned. In spite of the tremendous critical literature, not a single experiment presenting evidence favorable to ESP is specifically mentioned or listed as being inadequately controlled. Even the experimental methods, which are presented only in barest outline, are not compared for adequacy of control of sensory cues. Much space is given to the effectiveness of sensory cue control in the better methods, none to the absence of control in others. A paper by one of the authors (Pratt, pp. 65 and 84) is cited as exemplifying a special statistical procedure. In Appendix 10, this statistical method is presented correctly, and it is stated that the method has been used incorrectly "in past applications." The fact that Pratt was the writer who used the method incorrectly in the past application in question is not mentioned.

An unusual statement of scientific logic may be quoted: "At the outset it was indicated that, according to scientific logic, if there is *some* of the evidence which remains inexplicable by *all* of the counter-hypotheses taken singly and jointly, the hypothesis under investigation is thereby established by that fact alone" (p. 154). If one could be certain that *all* of the counter-hypotheses had been proposed and found wanting, and if ESP were merely a negative hypothesis, this statement would be

more nearly true. However, it is impossible to know when the first condition is satisfied, and the authors themselves object to the statement that ESP is a negative hypothesis (p. 149).

In the last section of the book, relationships of ESP to other variables are discussed. The absence of a critical approach is even more apparent here than in earlier chapters. For example, on pages 273-278, differences in results obtained with the open-matching and other techniques are discussed. Although the open-matching procedure involves no visual screening and the subject handles all of the cards, no mention is made of the probability of the open-matching method producing higher scores as a result of uncontrolled sensory cues. Instead, an elaborate ESP-perceptual hypothesis is proposed.

In spite of the scientific form, including tables, appendices, glossary, etc., the work will not be of much value to scientific workers, principally because of its inadequate presentation of experimental methods and criticisms. Its propaganda value among nonscientific readers is another matter, but that need not be discussed here.

DOUGLAS G. ELLSON.

*Stanford University.*

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EATON, H. S. (Compiler). Semantic frequency list for English, French, German, and Spanish: a correlation of the first six thousand words in four single-language frequency lists. (Issued by the Committee on Modern Languages of the American Council on Education.) Chicago: Univ. Chicago Press, 1940. Pp. xxi+441.

JERSILD, A. T. Child psychology. (Rev. and enlarged.) New York: Prentice-Hall, 1940. Pp. xiii+592.

WHITE, S. E. The unobstructed universe. New York: Dutton, 1940. Pp. 320.

YOUNG, K. Personality and problems of adjustment. New York: Crofts, 1940. Pp. x+868.

—. L'hygiène mentale et l'éducation. (Premier Congrès annuel, 19-22 juin, 1940.) Ottawa: Les Éditions du Lévrier, 95, Avenue Empress, 1940. Pp. 187.

## NOTES AND NEWS

DR. JOHN G. JENKINS, chairman of the department of psychology at the University of Maryland, will serve there on a half-time status this year. The balance of his time will be devoted to work as Director of Research for the Committee on Selection and Training of Aircraft Pilots, established last year by the National Research Council.

DR. RICHARD J. VAN TASSEL, associate professor of psychology at Miami University, Oxford, Ohio, died on August 23, 1940, of a heart attack suffered at his summer cottage in Wisconsin. Dr. William C. Biel has been appointed assistant professor of psychology at Miami and will take over some of Dr. Van Tassel's duties.

DR. JOSEPH E. BREWER has been awarded a General Education Board post-doctoral fellowship for one year beginning September 1, 1940, to continue his studies at the University of Illinois under the direction of Dr. Harold H. Anderson, with whom he has been working for the past three years as research assistant. He will continue research in the program of measuring the classroom behavior of children and of teachers in the elementary school.

DR. C. R. CARPENTER has been appointed associate professor of psychology at the Pennsylvania State College. He will continue to coöperate with the School of Tropical Medicine at San Juan, Puerto Rico, in work with the Santiago Primate Colonies, holding a nominal appointment as research associate.—*Science*.

DR. VERNE VINCENT CALDWELL, professor of psychology at the Oregon College of Education, has been appointed dean of the General Extension Division of the Oregon State System of Higher Education.—*Science*.

H. H. HSIAO, head of the department of psychology of the National Central University, Chungking, China, has written the following letter to the *Bulletin*: "Owing to the governmental control of foreign exchange, we have been out of touch with new literature in the field of psychology almost since our removal to Szechwan, but there is an increasing need for up-to-date information in our research work here. We wish that you will render us the much-needed help by sending your donations in the form of reprints, periodicals or test forms."

THE Boston Psychoanalytic Institute announces three additional Sigmund Freud Memorial Fellowships for Psychoanalytic Training, to begin in September, 1941. These fellowships are open to graduates of a recognized medical school who have had at least one year of general hospital training and two years work in psychiatry, and cover tuition only.

One additional fellowship for training in applied nontherapeutic

psychoanalysis will be open to those who have a Ph.D. or equivalent degree in the field of anthropology, sociology, pedagogy, etc.

Further information may be obtained from Dr. M. Ralph Kaufman, Chairman of the Educational Committee, Boston Psychoanalytic Institute, 82 Marlborough Street, Boston, Massachusetts. Applications close on February 1, 1941.

THE publishers of *Philosophic Abstracts* have announced for Summer, 1941, publication *The dictionary of philosophy*, under the editorship of Dagobert D. Runes and a number of collaborators. The volume is to cover metaphysics, ethics, epistemology, logic, philosophy of religion, esthetics, philosophy of law, philosophy of education, social philosophy, and philosophical psychology. Communications regarding the volume should be addressed to Philosophic Abstracts, 570 Seventh Avenue, New York city.

THE following is taken from a special release from the National Coördinating Committee on Education and Defense:

The expanding program of national defense increasingly affects every level of education. Not only will the schools and colleges be ready to serve governmental agencies, but in the present situation they will find ways to make education increasingly effective to meet existing and potential social needs.

Recognizing this fact, Dr. George F. Zook, president of the American Council on Education, and Dr. Willard E. Givens, executive secretary of the National Education Association, called a meeting of the National Coördinating Committee on Education and Defense in Washington, D. C., on August 5, to consolidate and develop the facilities already enlisted in the defense program.

The Operating Committee, appointment of which was approved at that meeting, held its first meeting on September 6, with Drs. Zook and Given as co-chairmen and L. H. Dennis as recording secretary. The Committee's activities are to include: (1) immediate and continuous representation of organized education for effective coöperation with the National Defense Council, the Federal Security Agency, and other governmental divisions; (2) stimulation and coördination of the efforts of educational organizations and institutions in projects related to the national defense; (3) dissemination of information regarding defense developments to educational organizations and institutions; (4) maintenance and improvement of educational opportunities essential in a long-range national program.

It is the hope of the 18 members of the Committee that it will serve as the medium through which practical policies of relationship between education and defense agencies may be applied and through which the varied activities of educational organizations may be coöordinated.

The 55 member organizations of the National Coöordinating Committee on Education and Defense include the American Psychological Association.

THE Social Science Research Council has announced that post-doctoral research training fellowships, pre-doctoral field fellowships, and grants-in-aid of research in the social sciences are being offered for the academic year 1941-1942. Applicants may write to Laura Barrett, Secretary for Fellowships and Grants-in-Aid, 230 Park Avenue, New York City, for blanks and details. It is important that those requesting application blanks for fellowships specifically indicate their age, academic qualifications, and tentative field plans; those desiring grants-in-aid should indicate previous research experience, nature of project, and amount of aid required. The closing date for the receipt of applications on blanks to be secured from the Secretary is February 1, 1941, in the case of fellowships, and January 15, 1941, in the case of grants-in-aid.

UNDER the auspices of the Division of Anthropology and Psychology, the National Research Council held a conference on "Psychological Factors in National Morale" in Washington, November 2 and 3. The conference was attended by 20 psychologists and a few representatives from various agencies of the government concerned with National Defense. The psychologists present, aside from G. W. Allport, who acted as Chairman, were: J. R. Angell, R. Benedict, R. A. Brotemarkle, H. Cantril, L. Carmichael, E. Culler, L. Doob, H. B. English, P. R. Farnsworth, G. H. Gallup, W. S. Hunter, J. G. Jenkins, R. Likert, L. G. Lowrey, W. R. Miles, H. A. Murray, G. A. Murphy, R. S. Woodworth, and R. M. Yerkes.

THE introduction of compulsory military service in the United States has stimulated a number of psychologists to plan studies of the change in attitudes produced by a year of such training. It seems likely that coöperation between persons doing such studies may lead to greater effectiveness than isolated investigations. The S.P.S.S.I. Committee on Morale in a Democracy therefore offers its services in putting in touch with each other psychologists who plan such research. Anyone planning studies of the effect of conscription on attitudes is invited to write to Gardner Murphy, College of the City of New York, Chairman of the Committee, for the names of others working in this field.

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